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BUDDHIST INFLUENCE IN THE GOSPELS.[1]

THERE are four cases in which after long consideration I have become convinced that Buddhist influence in the Gospel stories cannot be denied. This conviction in the first two cases is based essentially upon the latest exposition from Edmunds's pen.[2]

1. The Buddhist story of the *old saint Asita and his glorification of the child Buddha* has often been compared with the story of Simeon in the temple (Luke ii. 25f). It is found not only in the post-Christian biographies, in Ashvaghosha's Buddhacarita, in the Lalitavistara, in the Mahâvastu and in the Nidânakathâ, but also in the Suttanipâta, one of the oldest Pâli sources which takes us back several centuries before Christ's birth.[3] The saint Asita on a visit to heaven observes that the divine hosts are shouting for joy and learns from them that the Saviour of the world has just been born upon earth. Thereupon he hastens—in a miraculous manner, as Simeon to the temple—to the birthplace of the child, who shines with a brilliant splendor. He takes the child on his arm, calling it the highest and most glorious of mankind, but soon bursts into tears because he will have to die before the child will have attained Buddhahood.

[1] This article comprises pp. 47-61 of the first chapter of Dr. Garbe's *Indien und das Christentum* (Tübingen, J. C. B. Mohr, 1914. English translation by Lydia G. Robinson in preparation, to be published by The Open Court Publishing Co.)

[2] *The Monist*, XXII, 1912, pp. 129f.

[3] Accordingly it is quite impossible to find Christian influence in the Buddhist story as does Fausböll in *Ausgabe des Suttanipâta*, p. vi. Cf. also Oldenberg in the *Deutsche Rundschau* of January, 1910, p. 30 note.

In spite of some discrepancies the agreement of this story with the story of Simeon is greater than in most other cases; but it would not be sufficient by itself to render probable the dependence of the Christian upon the Buddhist story. It has been justly asserted that the fundamental idea, namely that an old man as representative of old times does homage to a child who is to introduce a new era, is to be explained on both sides from universal human experiences. Even Edmunds admits the weakness of the Asita-Simeon parallel if regarded by itself; "but its strength consists in its organic connection with the Angelic Hymn, both in Luke and the Sutta Nipâto."[4]

Here to be sure first of all a correction is necessary. The connection of the Asita-Simeon parallel with the glorifying of the heavenly hosts is not "organic" on both sides, but only in the Pâli text; whereas in Luke the story of Simeon does not stand in an intrinsic connection with the glorifying of the heavenly hosts (ii. 9-14) but only in its immediate neighborhood separated by seven verses. But even this coincidence is so remarkable that we can not think of it as accidental. Moreover in the Buddhist source Asita's knowledge of the birth of the miraculous child which he learned through the rejoicing of the heavenly hosts is much better accounted for than that of Simeon in Luke.

The text of the heavenly rejoicing in the Suttanipâta (in Edmunds's translation) is as follows: "The *heavenly hosts* rejoicing, delighted, and Sakko the leader, and *angels* white-stoled seizing their robes, and *praising* exceedingly, did Asito the hermit see in noonday rest. [He asks the angels why they rejoice, and they answer:] The *Buddha-*

[4] *Op. cit.*, p. 131. Pischel in his *Leben und Lehre des Buddha* (2d ed., p. 18) regards a loan on the Christian side as very probable without taking this connection into consideration; and even Winternitz, who thinks very little of loan hypotheses ordinarily, declares the Asita-Simeon parallel in itself "one of the most striking Buddhist parallels to the Gospels" (*Gesch. d. ind. Litt.*, II, 1, p. 75, note 2), and regards it as (p. 281) "to some extent probable that the Buddhist legend was known to the author of the Christian story."

to-be, the best and matchless Jewel, *is born for weal and welfare in the world of men,*[5] *in the town of the* Sâkyas, in the region of Lumbinî. Therefore are we joyful and *exceedingly glad.*" Thereupon Asita hastens to do homage to the new-born saviour, as do the shepherds in Luke.

It is impossible to expect a closer coincidence of the Buddhist legend with Luke ii. 9-32 under the previously established hypothesis that the former traveled by oral communication from Bactria into Syria. We ought rather be surprised that so many characteristic features of the Buddhist original have been preserved in the Christian recasting.

2. The *story of the temptation* belongs to the most striking and the most generally discussed parallels between Buddhist literature and the Gospels; for an incarnate devil is not made to appear on earth in any other place in the Bible, but constantly in the Buddhist canon. According to the narrative in Matt. iv. 1f, and Luke iv. 1f, after Christ has fasted for forty days in the wilderness and is overcome by hunger, the devil demands that he change stones into bread, that he throw himself down from the pinnacle of the temple and worship him, the devil, in order to receive as reward in return the kingdoms of the world and their glory. Since all the temptations are in vain the devil withdraws ἄχρι καιροῦ, "for a season," Luke iv. 13. (Luther translates, "*bis zu einer günstigeren Gelegenheit.*").

Buddhist literature is extraordinarily rich in similar tales in which Buddha is tempted or injured by Satan (Mâra), now in one way and now in another. But for our purpose there are only three passages to consider from the earliest Pâli literature (the Padhâna-sutta, the Samyutta-nikâya and the Mahâ-parinibbâna-sutta), and these in a surprising way form a counterpart to the Christian temptation story and invalidate the reasons which have

[5] It is an unfortunate idea of Edmunds to explain the words in Luke ii. 14, ἐπὶ γῆς εἰρήνη ἐν ἀνθρώποις εὐδοκία as a literal translation of the Pâli text, *manussa-loke hita-sukhatâya jâto.*

previously been brought forward against the loan hypothesis.

The negative standpoint is best presented by E. Windisch with his usual clearness.[6] But that his expositions nevertheless leave room for the opposite conception has been emphasized by Van den Bergh[7] who closes his chapter on the story of the temptation with the following words: "The striking coincidences....: the temptation in solitude by the devil who appears personally (Mâra) and speaks with Buddha, following close upon the hunger caused by a long fast;[8] the triumphant rejection of this temptation, whereupon the Evil One decides to await a more favorable season; the offer of the dominion of the world which is intended to keep the king's son from his higher calling. These are exactly the points which are already present in the ancient pre-Christian form of the Mâra story." Shortly before (pages 45-46), Van den Bergh had refuted the view that the corresponding legend of Parsism, which is nearer to primitive Christianity both geographically and dogmatically, and according to which (in Vendîdâd 19) the evil spirit tries to entice Zarathustra to worship him by the alleged offer of world dominion, offers a better point of comparison for the Christian story of the temptation. He believed that the parallel features among Christians and Zarathustrians appeared in much more diverse surroundings, whereas in the whole scheme of the Buddhist and Christian temptation stories there was more agreement; also that the Vendîdâd text does not deal with the dominion of the world but with the good fortune of Vadha-

[6] *Mara und Buddha*, Leipsic, 1895, pp. 214f.

[7] 2d ed., pp. 47-48.

[8] It is related in the Padhâna-sutta that Buddha in his exaggerated ascetic practices, of whose futility he later became convinced, had fasted almost to the point of death *before* the enlightenment, and therefore *before* the temptation which is of interest to us for our comparison and which took place directly after the enlightenment. Accordingly the fast serves the same purpose in the Buddhist as in the Christian legend, namely to account for the complete physical exhaustion of the Saviour as the point of attack for the tempter.

ghana, the reigning prince, which is held out to Zarathustra as a reward for forswearing the Mazda faith. I would like to add that in my opinion the dependence of the Zarathustra story on the Buddhist temptation story is not at all improbable. At any rate there are no chronological difficulties in the way of this hypothesis.

In the Avesta the evil spirit bears the epithet *mairya*, "destructive," which comes from the same root and is almost the same word as the Buddhist *Mâra*. But here we have one of the cases in which what seems probable at first glance is not correct; the two words *mairya* and *Mâra* stand in an etymological connection, it is true, but not in a historical connection. In the Avesta all evil beings are called *mairya*; and if one could suppose that this epithet is transferred to them from their commander yet the age of the Avesta writings in which it appears is at least in part removed beyond the suspicion of Buddhist influence.

To the words of Van den Bergh above quoted about the "striking coincidences" between the Buddhist and Christian legends Wecker[9] adds the question: "But where is the 'old pre-Christian' form of the Mâra story to be found which contains all these points or even most of them? There is no such story." To this it may be answered that there is indeed such a version, although it does not appear so distinctly in Van den Bergh's summary of the literature as in the combinations of passages from ancient Pâli literature recently submitted by Edmunds. This combination even furnishes two other striking coincidences and through these has mainly contributed to alter my standpoint on the loan question. That the ancient pre-Christian form of the Mâra story which corresponds to the Christian legend must be gathered together from different texts, need cause no hesitation, if we explain the influence on the Gospels

[9] *Christus und Buddha*, 3d ed., p. 21.

by the transmission of Buddhist tales from mouth to mouth and not by the utilization of a literary source.

The text of the passages which Edmunds places side by side with the text from Luke iv, is too copious for me to reproduce it here *in extenso*. Whoever would like to compare it entire must look it up in *The Monist*, XX, pp. 131-133. I limit myself here to what is essential for our purpose and observe in the first place that Edmunds's placing the passages side by side must be supplemented at the start by the account in the Padhâna-sutta[10] of the fast which Buddha had carried to the extreme limit. Only after this ought the story in the Samyutta-nikâya (Mâra-Samyutta, 2, 10) follow—the story of the temptation to assume worldly dominion and to change a substance—which I can not reproduce more tersely than in the words of Oldenberg:[11] "At one time the Exalted One was living in the land of Kosala, in the Himalaya, in a log hut. When the Exalted One was thus living retired in solitude this thought entered his mind: 'It is possible really to rule as a king in righteousness without killing or causing to be killed, without practising oppression or permitting oppression to be practised, without suffering pain or inflicting pain on another.' Then Mâra, the Evil One, perceived in his mind the thought which had arisen in the mind of the Exalted One, and he approached the Exalted One and spake thus: 'May the Exalted One, O Sire, be pleased to rule as a king; may the Perfect One be pleased to rule as a king in righteousness without killing or causing to be killed, without practising oppression or permitting oppression to be practised, without suffering pain or inflicting pain on another.' Buddha challenges him: 'What dost thou see in me, thou evil one, that thou speakest thus to me?' Mâra says: 'The

[10] Translated by Fausböll, SBE, X, pp. 69f, and better by Windisch in *Mara und Buddha*, pp. 9f.

[11] *Buddha*, 6th ed., pp. 356f; English translaton by William Hoey, London, Williams and Norgate, 1882, p. 312. The text has been translated in full into German by Windisch, *op. cit.*, pp. 107f.

Exalted One, O Sire, had made the fourfold miraculous power his own. If the Exalted One, O Sire, desired, he could ordain that the Himalaya, the king of mountains, should become gold, and it would turn to gold.' Buddha motions him away. What would it profit the wise man if he possessed even a mountain of silver or of gold? He who has comprehended sorrow, whence it springs, how can he bend himself to desire?....Then Mâra the Evil One said, 'The Exalted One knows me, the Perfect One knows me,' and disconcerted and disheartened he rose and went away."

Here Edmunds joins the story of the "Temptation to commit suicide" from the Mahâ-parinibbâna-sutta.[12] When Buddha attained enlightenment at Uruvelâ on the bank of the brook Nerañjarâ under the tree of knowledge, and had exterminated all earthly desire within himself, he had finally escaped Mâra's power. The Evil One was well aware of this; but he still cherished the hope that he might retain the rest of mankind in his fetters, and therefore sought to prevail upon Buddha to abandon the idea of proclaiming the truth he had obtained and to depart at once from this life. He came to him and said, "May the Exalted One, O Sire, enter now into Nirvâna; may the Perfect One now be pleased to expire. Now is the time, O Sire, for the Nirvâna of the Exalted One." But Buddha repulsed the temptation and declared that he would not put an end to his life, until he had obtained enough disciples, monks, nuns, lay brothers and sisters, to assure the continuance of his doctrine and of holy conduct.

[12] In two passages of this work which "resembles our Gospels more than any other text of the Tipitaka" (Winternitz, *Gesch. d. ind. Litt.*, II. 1, p. 31; Oldenberg, *Die Literatur des Alten Indien*, p. 94), namely II, 7-9, and III, 34-35, Mâra made the appeal to Buddha to enter into Nirvâna: once directly after the enlightenment, the second time three months before Buddha's death. Only the first temptation of this character has really any meaning, the second is a foolish repetition; but in our text the second stands in the first passage and refers to the earlier temptation of which Buddha tells his favorite disciple Ananda in the later passage. Edmunds has incomprehensibly inserted the first

From piecing together these passages in the text we have the following coincidences besides those established by Van den Bergh and cited above (page 484). Both in the ancient Buddhist legend and in the corresponding Christian account Satan attempts to lead the redeemer astray (1) to change one substance into another by the use of witchcraft and (2) to put an end to his life.[13] Thus we have in all too many coincidences to be satisfied with Oldenberg's explanation that in both cases the same obvious motives have given rise to the corresponding narratives.[14]

As far as the temptation to change, respectively, stones into bread and the Himalaya into gold is concerned, the Christian version of course represents the incomparably more natural supposition because of the situation. If the Gospels were as old as the Buddhist source we would not hesitate a moment to declare the Christian version the original one and the Buddhist a distorted copy. But since the Buddhist source is centuries older, if we are convinced of the dependence of the Christian temptation story upon Buddhist prototypes, we must accommodate ourselves willy nilly to the view that the genuinely Indian grotesque and exaggerated idea was constructed more simply and more naturally on Christian ground, and adjusted to the situation.

3. To Peter's *walking on the water* (Matt. xiv. 25f) we have a parallel[15] in the introduction to Jâtaka 190, where the similarity is far greater than usual. There we

passage of the text, which of course should be replaced by the second, whose substance I here give in brief, so far as it treats of the earlier temptation.

[13] Edmunds, *ob. cit.*, p. 133.

[14] *Buddha*, 6th ed., note 2.

[15] The first forgotten discoverer of this parallel was H. Wenzel in "Coincidences in Buddhist Literature and the Gospels," *Academy*, Jan. 12, 1889, p. 27. The same was pointed out later by Max Müller in the article "Coincidences" in *Last Essays*, I, p. 284f. (Reprint of a lecture of the year 1896. See *Fortnightly Review*, N. S., LX, p. 48f.) and independently of him also by Van den Bergh in *Indische Einflüsse*, 2d ed., p. 52, since the Dutch original of this book appeared in the same year (1901) as Max Müller's *Last Essays*.

are told of a disciple who wished to go to Buddha one evening and failed on his way to find the ferry boat by the bank of the river Aciravatî. Trusting implicitly in Buddha he stepped upon the water and walked on it as on solid ground as far as the middle of the river. There he awoke out of the joyful thoughts on Buddha in which he had been engrossed, observed the waves with terror and his feet began to sink. But he forced himself to a renewed contemplation and by its power succeeded in gaining the other shore and his Master.

In comparing this Buddhist story with the Christian, the corresponding feature that Peter began to sink as a consequence of his little faith just as Buddha's disciple did because of his wavering contemplation, is much more important than the theme of walking on the water. It seems impossible to account for this most remarkable coincidence in any other way than by a loan. And here it is only on the Christian side that a loan can be considered, although the Buddhist story is not in the Jâtaka itself but in the introduction to it and hence belongs to the so-called "Stories of the Present" whose composition brings us down to the fifth century A. D. But these "Stories of the Present" come from the same author as the "Stories of Other Births," i. e., as the prose commentary to the ancient Pâli verses; and the kernel of the Jâtaka prose is considered by the most eminent critics as far as the subject-matter is concerned to be in general as old as the verses.[16]

The idea that men can walk on the water by virtue of the power of their asceticism, or on account of their virtue or piety, is very old and wide spread in India, and it is entirely foreign to Judaism. If walking on the water has become a favorite theme in later Christian legends, it is on account of Peter's walking on the water.

[16] Oldenberg, *Nachr. d. K. Ges. d. Wiss. zu Göttingen*, 1912, p. 212; Jarl Charpentier, *Zeitschr. der Deutschen Morgenländischen Gesellschaft*, LXVI, pp. 41-42, note; Winternitz, *Gesch. d. ind. Litt.*, II, 1, p. 95.

Edmunds[17] finds the earliest record of this idea in Indian literature in Sûtra 6 of the Majjhima-nikâya, one of the oldest Pâli works. There Buddha names the ability to walk on the water as one of the powers of a pious monk. It is also frequently related in the sources that Buddha himself walked or flew over the water. The idea that extraordinary people were able to walk or drive in a vehicle on the water, however, belongs not only to the India of Buddhism but also to that of Brahmanism. In the Mahâbhârata the same thing is told of the pious and virtuous kings Dilîpa (VII, 2267, 8) and Prithu Vainya (VII, 2402).[18]

Edv. Lehmann, who otherwise does not believe in New Testament borrowings from Buddhism, says with reference to the parallel here discussed:[19] "If one were to assume a loan here perhaps it would not be a false conclusion; Christianity does not become any poorer by forfeiting this story." Certainly not; on the contrary! What sincere worshiper of Christ would not be glad to miss from the accounts of his life this and certain other useless sorceries which corresponded to the taste of those ancient times! I am thinking of the changing of water into wine at the marriage in Cana (John ii. 1f); of the coin that Peter

[17] *The Monist,* XXII, pp. 134-135. R. Otto Franke was good enough to give me the exact quotation in the edition of the Majjhima-nikâya by V. Trenkner and R. Chalmers (Pali text society). There it reads (I, p. 34, l. 16f): "When the monk wishes, 'I would like to be able to practice the various powers of holiness, namely to change from my one person into a multiplicity, from a multiplicity back into unity, to make myself visible or invisible, to pass unhindered through walls, rocks and mountains as through empty space, to dive into the earth as into water and out again, and *to walk upon the water as upon the earth without it yielding*'.... then he may become perfect in moral discipline...." Franke makes the following comment: "The passage is not unique, but occurs frequently in the Majjhima-nikâya and is older than the M.; for this scheme of the *iddhividhâ* (all the miraculous powers collected together) belongs to the plan of salvation which forms the kernel and foundation of the whole Digha-nikâya, the oldest (at least prose) text of the canon. We find it in Digh. II, 87 and correspondingly in all the following D.-Suttas up to XIII." See also Franke's essay, "Das einheitliche Thema des Dighanikâya," *Wiener Zeitschrift für die Kunde des Morgenlandes,* XXVII, pp. 198f, 276f.

[18] E. Washburn Hopkins in *Proceedings of the American Philosophical Society,* XLIX, No. 194, 1910, p. 38.

[19] *Der Buddhismus,* p. 88.

was said to have found in the mouth of a fish which had first to be caught for this particular purpose (Matt. xvii. 27); of the cursing of the fig-tree so that it withered, because it bore no fruit at a time when there could not be any figs (Matt. xxi. 19f; Mark xi. 13, 20, 21); of the ugly expulsion of demons into the herd of swine that rushed into the sea and were drowned (Matt. viii. 30f; Mark v. 11f; Luke viii. 32f) and other similar instances.

4. The *miracle of the loaves* (Matt. xiv. 15f; Mark vi. 35f; Luke ix. 13f) has a parallel Van den Bergh does not mention but which I am compelled to count among the striking ones. In the Introduction to Jâtaka 78,[20] it is related that Buddha fed first his five hundred disciples and then all the inmates of a cloister with one loaf which was put in his begging bowl, and that nevertheless a great deal of bread was left over which was thrown into a hole by the gateway. Here too the similarity to the New Testament story is less noteworthy on account of the miracle as such but much more so on account of the feature that here too twelve baskets of fragments *were left over* and on account of the *repetition of the number five*. In the New Testament accounts, as we know, five thousand people were fed with five loaves (and two fishes). The smaller number five hundred in the Buddhist story has the advantage of originality in comparison to the five thousand in the New Testament. And it especially happens that "the number five hundred is eminently Buddhist, as we could prove by numerous texts."[21]

These reasons are sufficient in my estimation to look upon the Buddhist story as the source of the Christian

[20] Max Müller, *Last Essays*, I, pp. 284f; Franke, *Deutsche Lit. Ztg.*, 1901, 2760; Edmunds, *Buddhist and Christian Gospels*, 4th ed., II, pp. 253f. Max Müller has overlooked the fact that this parallel and the one discussed directly before it are not in the Jâtaka text itself, but in the later introduction to the Jâtaka.

[21] Edmunds, *op. cit.*, II, p. 256. R. Seydel has made the same observation in *Das Evangelium von Jesu*, p. 285, note 238, with reference to 1 Cor. xv. 6.

parallel, and at the same time to regard the material of the former as much older than its literary form in the "Story of the Present" of the fifth century A. D.

If we recognize Buddhist influence on the New Testament in these four cases[22]—and I think we must recognize it—we can hardly deny the probability that not only here but in other places in the Gospels the same influence has been at work where it has not shown the same degree of probability. Theology ought not to find any difficulty in making friends with this idea, for the eternal value of Christianity can suffer no injury from it.

Whoever admits the much stronger foreign influences on primitive Christianity on which I have dwelt in my Introduction[23] can also accept without danger to the Christian faith the harmless Buddhist influences on Gospel narratives. But it is very surprising that many theologians who are convinced of the deep-seated Hellenistic influences on the New Testament can not make up their minds to accept any influence on the part of Buddhism. Do they perhaps scent a greater danger?

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[22] Also the expression *τροχὸς τῆς γενέσεως* in James iii. 6, is, as Schopenhauer has already remarked, so specifically Buddhist and so generally Indian that it is difficult for me to account for it in any other way than by Indian derivation. The whole text of the verse sounds Buddhistic, for Buddha too compares the sense organs to flaming fire in one of his earliest speeches (Mahâvagga, I, 21, 2, 3: *jivhâ âdittâ*, "the tongue stands in the midst of flames," corresponds literally to the *ἡ γλῶσσα πῦρ* in our passage in the Epistle of James). That the *τροχὸς τῆς γενέσεως* is derived from the Orphic mysteries, as many theologians have thought (Clemen, *Religionsgesch. Erklärung d. Neuen Test.*, p. 58), is just as improbable as Edmunds's alleged Babylonian derivation (*Buddhist and Christian Gospels*, 4th ed., II, p. 263) following Count Goblet d'Alviella.

[23] Translated under the title "Gentile Elements in Christianity," in *The Monist*, July 1914, p. 454.

UNITY OF WORLD-CONCEPTION.

THOSE to whom philosophy makes no appeal, to whom metaphysic is anathema, and mysticism a mirage, turn hopefully to science as a guide to sensuous phenomena; and, in so far as the method of science is followed, no better aid can be found to explore the wonders of the world. Science, treading on the firm ground of experience and experiment, can travel far into the realm of nature, opening up at times new avenues of approach which even science hesitates to enter, and pursuing patiently her path towards some half-acknowledged and ever-receding goal of knowledge which the "man in the street" calls the "riddle of the universe." To speak of the arcana of nature as a riddle to be solved is, no doubt, to the humble student who brings his offerings to the temple of Isis, little short of blasphemous; but it is characteristic of the times to regard nothing as sacred and insoluble.

Now, the mariner who sails the high seas knows full well that to reach a known port in a distant land true orientation and correct charts are necessary adjuncts to his skill in navigation. Without these he could not attain his goal except at hazard and by hard adventure, and of these orientation is the chief essential. If he be navigating a sailing vessel across the trackless ocean, the general direction of his voyage will be towards his port of call; but many and devious may be the tacks against foul winds, away from the true direction of his objective, ere he sights his landfall. If finally, by consulting his chart, he decide that his goal may be reached by following

a definite course for a certain period of time, and he finds this indeed to be *true,* then it follows that all his previous positions pricked on the chart must have been correctly determined; and these positions are then made good. Science advances in this way.

This simple illustration, however, is applicable only to known ports and known routes; it cannot apply to the unknown. It follows on this analogy that any attempt to seek the unknown through the known processes and principles of the logic of science is a hazardous and speculative undertaking, since argument easily may be carried beyond the legitimate field of *verae causae.* Nevertheless, this is what many adventurers, trusting to a higher instinct, which sometimes inspires and sometimes misleads men, are trying to do at the present day. They follow confidently in the beaten path of science, in the hope that, eventually, they may outstrip her and win to the promised land. What that land shall contain for them is merely adumbrated by philosophers and men of science alike: if it were known, there would be no need to seek it except as a recapitulation of experience; being unknown, they await the dawn of another day, and yet another day, when, perhaps, this object of knowledge—dimly perceived—shall be fully determined.

But why, one may ask, should one postulate any such object of knowledge?

Should not we be content with things as we know—or think we know—them to be, rather than speculate on things as we would have them to be, since the former is the criterion of science? Is there any appeal to supersensuous phenomena? "The first and greatest question which philosophy has to resolve, in its attempt to make out a Kosmos," said Sir John Herschel in one of his *Popular Lectures* (p. 474), "is whether we can derive any light from our internal consciousness of thought, reason, power,

will, motive, design—or not: whether, that is to say, nature is or is not *more interpretable* by supposing these things (be they what they may) to have had, or to have, to do with its arrangements. Constituted as the human mind is, if nature be *not* interpretable through these conceptions, it is not interpretable at all." We know that migrant birds and certain aboriginal savages possess an innate power of orientation, and we account for this in a simple and natural way: by heredity, or adaptation to environment. We know, too, that man always has possessed in a greater or lesser degree an innate faculty of apprehension, which orients his mind towards something beyond himself in the direction of psychic experience and development. The world may satisfy his material needs—and does for most men, who find it a hard taskmaster—but in the realm of mind material needs are merely supporters of the intellect, as oxygen is a supporter of combustion. The trained mind —the intellect—ranges far beyond the common objects of thought and is nourished by ideas and ideals. There is, in short, in every healthy and simple mind, an innate sense of psychic function residual in consciousness, which men —certain idealists, at least—call the soul. To find a fitting habitation for the soul in the universe of mind and matter is the ideal quest of truth.

The restless spirit of modern thought, which courts disaster by the very excess of its energy, is held in check—no less than inspired—by the advance of science in search for a universal formula that shall interpret the significance of life and mind and correlate the forces of nature. Logically, such a formula is readily reached by appeal to the law of continuity, based perhaps on reciprocity of force—a logical extension of thought in a cosmos of recurring phenomena. But science is impatient of metaphysical speculation and builds its hypotheses on surer ground. It cannot altogether discard the metaphysic of

experience—as witnessed in the Newtonian laws of motion with their metaphysical groundwork—but it limits this indiscretion to the debatable borderland of mathematical proof. The groundwork of science is a closed system of thought; and, therefore, science never can usurp the primacy of thought, which transcends experience.

It is well for the advancement of science that this closed system of thought should be rigidly fenced and jealously guarded by constituted authority, lest speculation—as to what may lie beyond—lead to error and confusion. It is well, too, for the advancement of man that this closed system should not imprison thought within the iron bars of scientific materialism (epiphenomenalism) lest visions of freedom—for his insurgent soul—do but mock his highest aspirations, and so enslave his will to the exclusive exercise of his material functions.

In the realm of mind there are no barriers to freedom of thought; the attributes of mind are consentaneous with the established order of things. But no two men of deep and original thought see things precisely in the same order and as having the same value of mind; each will have his special connotation and his individual interpretation. Hence arise controversy and mutual misunderstanding. The great reconcilement between thought and things is, by common consent, adjourned to the Greek Kalends.

Nevertheless, the obvious unity of nature demands an interpretation of the cosmos in terms of finite mind, the exclusive heritage of man. A true system of thought should represent synthetic unity of world-conception and a monistic view of substance in correlated terms of mind.

Man is born to a godly heritage, the fruit of labor. Alone, among all forms of life on the earth, he enjoys this distinction, this dignity, this power: to labor towards some desired end, *consciously* and of set purpose; to create ideals of perfection and strive to attain these ever elusive goals.

He raises his home on the ashes of the past, which the wind scatters. He receives thoughout his life more than he ever can repay, and thereby enjoys a balance of benefit. Time passes, and he too falls from the ranks of men marching towards their destiny; but, in passing, the spirit of man is handed on and recast from generation to generation.

Change and decay follow in natural order; each change serves its purpose for a time and is cast aside like a useless garment. Each generation stands midway between the past and the future; each regulates the periodic equilibrium of progress in the subcontrary movement of thought. Nature has a way of equalizing matters, a veritable force in organic evolution. That which never changes, but becomes only more conscious of itself, is the spirit of man, which inspires all his actions. Hence arise awareness and knowledge of self.

The torch of knowledge is passed from hand to hand, and burns brightly or dimly in the handling of it; at times it is all but extinguished. To dwell in darkness is to live a stunted life devoid of color and beauty; to rekindle the torch is beyond the power of ignorance. The torch of knowledge is a sacred flame, and those who serve nobly in its temple enjoy a distinction beyond the recognition of princes; posterity acclaims them. Knowledge nourishes and advances thought. Like the sun, it sets all things in motion; it illumines all objects of perception; it brings forth fruit in good season.

Thought is the fruit of the mind. Beautiful thoughts live for ever and purge the mind of evil communings. Law and order are evolved from careful culture of the highest thought.

Out of the chaos of unrelated things (such as cows and cowslips), of ignorance, of blank mind, there arise here and there threads or filaments of knowledge which, on being carefully traced, are found to link up facts that bear

a family likeness to one another—facts which thereby are said to be correlated, the sum and synthesis of which is true knowledge. The nervous system of man, apart from his body, thus may be pictured. The animal, vegetable and mineral kingdoms are examples of this arrangement of ideas. The solid, liquid and gaseous character of substances are other useful generalizations for grouping together co-ordinated factors of knowledge.

Our outlook on the world, too, is greatly simplified and extended by regarding its main natural divisions apart and in their inter-relationship—the land, the sea, the air. These also may be called kingdoms.

There are kingdoms, then, of the earth and of mind. Once the order of a series of allied facts is perceived, it becomes possible to establish a law by which these can be explained and related to other groups of facts. By this means we recognize families of facts, or categories of thought, which can be isolated, or set apart, for the purpose of closer study; to confuse these, by mixing them together, is to lose all trace of logical consistency.

The highest minds of all time have been engaged in this fascinating pursuit. First warily, by means of hypotheses or assumptions—mere guesses, sometimes—to account for what is not clearly understood, these men of thought and leading have created out of the chaos of things, dimly perceived, a structure (or framework) of logical reasoning (or simple formulas) which links up and groups all like facts and principles; but, until the idea or spirit which explains or vitalizes the meaning and purpose of this structure is discovered, it remains a dead thing: it cannot live in the race-memory. Monuments of patient research such as these are not often built up; but, when successfully raised, they remain—for centuries, perhaps—landmarks in the maze of thought.

Now the ideas that explain and vitalize many things that happen on the land, in the sea, and in the air, can be indicated briefly by two pass-words which carry us very far into debatable kingdoms of thought. These are the idea of *evolution* and the idea of *circulation.*

The idea of evolution is mainly associated with development of structure, or form: the external aspect of anything. The idea of circulation is associated with the function of elements composing anything: the internal flux of things undergoing a change of state, which usually is in the direction of complexity and expansion; as a butterfly from a grub, a torrent from a cloudburst, a revolution from an ideal. In this natural sequence—seed, flower, fruit— we perceive a *continuity of idea* in the same plane of thought, by which we come to recognize general principles of growth and development. If natural phenomena —things that happen in the orderly course of nature—were not uniform and constant, we could not reason logically, and therefore would be unable to discover any laws; but we rely absolutely, and stake our lives daily, on the uniformity and constancy of natural phenomena, and so escape shipwreck at sea and many perils by land in our familiar intercourse with nature.

There is, on the whole, a progressive evolution of forms from simple to complex, from indefinite to definite, both in things that live and things that are inanimate; and generally it will be found that the change of state, or degree of modification, is from a lower to a higher form of structure and function—and by higher form we mean a state better adapted for survival or service in the purposes, or apparent ends, of nature. From this broad generalization we derive the useful aspects of our subject which are known as the *organic* and the *inorganic* realms of organization, in the building up of the world: things which live and pass on *life,* and things which change or modify their

material composition. Thus, the fundamental antithesis in nature is *mobility or motion* (life and movement) and *rigidity or inertia* (complexus of matter); and the interplay between these dual forces in nature enables us to formulate a physical basis for phenomena.

Nature knows no rest. There is a constant flux and whirl of particles in all movements of matter, an interchange of properties between the elements of substance. But with this molecular motion we need not concern ourselves, beyond noting the fact that—for science—it exists; it is enough for us to have grasped a working principle, universally applicable, which underlies the study of terrestrial phenomena and illustrates the countless phases of circulation.

Restless nature moves on the face of the waters, which leap towards the moon in its path and sway towards the sun, by which tides are caused. Restless nature draws up bountiful supplies of vapor, which condense in a cool atmosphere and flood the earth with life-giving properties; denude mountains and scour out valleys; loose everything like hounds from the leash, to run an appointed course. Gravity, density, temperature and pressure are the principal agencies through which the activities of nature are revealed to us—apart from growth and development: in the roar of the storm and in the sweet silence of the desert, in the circulation of air and ocean currents, in the transformation of all matter on the surface of the earth and in the mutation of living forms, passing through the familiar cycle of change and decay, life and death, movement and counter-movement.

This constant struggle to establish equilibrium between the interacting forces of nature is thus curiously shown to be paradoxical in the sense that equilibrium does exist everywhere and in everything; but with this qualification: it exists only for a moment of time in a potential state of

change. Like the see-saw of children, nature, poised on the fulcrum of motion—i. e., stability of motion—determines the sway, this way and that, of the counterpoised levers that constitute our children's plank. The fleeting moment of equilibrium—i. e., moment of momentum—is always on the point of being overcome by the forces of action and reaction; and in these fleeting moments we pass our individual lives. Time, then, in all spatial relations, is inseparably associated with the consentaneous action of symmetrical parts in any system: the longer the time, the greater the change in the evolutionary field.

Action, however, is of two kinds—determinate and indeterminate. An indefinite act is simply the continuity of some previous movement in the same direction, subject to some unforeseen check to its development or issue in the flux of things. A free act, on the other hand, may be defined as a movement, or the arrested development of a movement, directed towards or against some definite end or purpose. Nature—as we understand her—being intensely dualistic and teleological, thus offers a choice between order and chaos, chance and purpose, in our construction of a cosmos; and therein lie all the elements of confusion and controversy.

What we see in nature is what we, ourselves, posit. The so-called laws of nature are, in reality, the laws of man-made nature—of man's interpretation of natural phenomena: causes, sequences, and the like. Without some idea of these, we cannot truly grasp the inner meaning or structure of things, even of the simplest. Most of us, it is true, are content to take things as we find them, with the official label attached, and secrete them in our memory; but there are others, of a rebellious type of mind, in whom a divine discontent reigns—a rage for finding out *how* things are made and *why* they work, a reverence for nature, worship of beauty. To all such, nature appeals with irre-

sistible force. The world becomes a temple—the temple of Isis—not a dead museum, in which silence and contemplation, search and research, thought and study, are the proper attitudes of mind. To all such, life never can be devoid of interest, never can disappoint, *but never can satisfy,* because the pursuit of knowledge for itself lures them towards an ever-receding horizon beyond which truth dwells, forever beckoning.

* * *

The truth of everything that is true and the falsity of everything that is false are true and false, respectively, only in relation to prevalent criteria of knowledge. The movement and persistence of thought are as fluent as the flux of things. But although the critical value attached to the precise meaning of a given term may vary in time, the idea created in the mind by its spontaneous expression—i. e., mental presentation — has a tendency to persist. Thought, then, in its persistence, has the quality of momentum; and if in quantitative analysis a fixed ratio to the sum of things—a world-formula—be sought, it may be found to lie in certain general principles the root idea of which is self-evident and of universal application. Of such are the idea of gravitation and the idea of biogenesis (life from life). The precise terms in which at the present day these two problems of science may be stated are undergoing revision; but, whatever be the issue in detail, their qualitative significance as fundamental concepts of mind is not likely to be affected.

The ideal goal of knowledge can scarcely be reached, or even sighted, until physical science (cosmology) becomes more philosophical and catholic in its outlook on the objects of knowledge; and until philosophy, thus reinforced, adopts the method of science in examining the intimate nature of knowledge, in order to derive a uni-

versal (space) and fundamental (time) principle that may be expressed in the unity of a formula, or constant of function, in cosmogonic evolution. Some such controlling factor seems essential to a strictly monistic view of substance and in order to define the evolutionary process. This factor we take to represent, in principle, the coefficient of meaning, carried through all the operations of thought in their quantitative expression. Unity of idea must underlie and govern any rational system of organization; and this unity, in some form, must be capable of realization. For, if a thousand persons of diverse minds surrounded a perfectly conical hill, all might reach the summit, blindfold, by taking the line of greatest resistance. This guiding principle in nature we take to be *difference of potential,* in the macrocosm: *identity-in-difference,* in the microcosm.

Is such knowledge possible? All we can attempt, in this place, is to indicate a few directions in which this root principle may be traced.

Our philosophy of unity postulates universal law operating harmoniously and reciprocally in nature, based on the dynamics of force—as a mechanical conception—which is the motive-power of the cosmos.[1] The fundamental idea of force has not yet been fully stated by physicists. Force (they say), like energy, is a dual conception, involving not only that which generates, accelerates, or changes motion; but it involves also resistance. No force exists without resistance. Ultimately, all things resolve themselves into two incomprehensible factors, and their duality is reconciled by a mysterious underlying unity which transcends our conception—or, at least, experience—both in living and non-living matter: the persistence of energy and the

[1] In general terms, force (or, at least, virtual force) may be defined as the ratio (MV^2) of mass and velocity in time and the constant of motion in space. In all apparent interactions between force and substance, the product (or sum) of the potential (or existing) energies remains unchanged. We therefore regard force and substance as two aspects of one and the same principle: conservation of work.

resistance of inert matter are essential to the existence of things, in the promulgation of science. Matter (in our view) is the vehicle of energy—its objective reality; and both are attributes of force, by virtue of which things are as they are—a self-compensating unity. Eliminate stability of motion, which binds atoms and molecules together in temporary groupings, *pari passu,* and the world would be pulverized into integral particles, each of which would be an electrostatic nucleus of energy. Destroy, other than to convert into energy, an appreciable aggregation of matter, and the cosmos would collapse—that is, the cosmos of science.[2] The mere fact of stability denotes static equilibrium, any subsequent disintegrations or mutations being in respect of time only. The conservation of energy and of matter are fundamentally essential to unity of world-conception. In how far may that unity be further expressed?

The lithosphere, or crust of rocks, and the hydrosphere, or system of oceanic waters, compose the planet earth, which in its orbit round the sun carries an atmosphere in its flight through space. The ether of space, which the unification of physics compels us to postulate, is the medium that transmits light and heat throughout the universe, and probably also is the seat of all electrical and kinetic forces. It even may be conceived as the immediate origin (or substratum) of all life and movement on the earth. The thousand millon stars and planets which we calculate to be in constant orbital motion float in this ethersphere, as jelly-fish in the ocean, and are composed of its primordial

[2] Thus, according to Newtonian principles: if the earth were to be blown into ten thousand fragments *at this instant,* every particle would return (or persist in a tendency to return) at some time or another to the point in space (relative to the solar system) which we occupy *at this instant.* Similarly, if an object be ejected at any time, it will describe an orbit which must pass through (or persist in a tendency to pass through, unless acted upon by some external force) the point in space (relative to the solar system) from which the expulsion occurred. This we take to denote reciprocity of force (by which a dynamical theory of motion is demonstrable) in mechanical causality: in other words, the mechanism of gravity, by which conservation of momentum is maintained.

world-stuff—i. e., modifications of ether. Thus, it may be inferred, electronic or corpuscular substance, interpenetrating all modes of matter, is the common basis or constituent of matter, the *Urstoff* in the laboratory of nature. The visible universe of matter contained in it is scarcely appreciable, in comparison with its immeasurable immensity. Nevertheless, the principles underlying the subtlest processes of nature are symmetrical, both in the infinitesimal and in the illimitable, in the atom and in the sidereal system; and this may be assumed, failing evidence to the contrary, in a system of thought proceeding from the known to the unknown by logical inference.

A molecular theory of matter, if less demonstrable than a molecular theory of electricity, is foreshadowed in the researches of Thomson, Larmor, and Lorentz. "For the most natural view to take, as a provisional hypothesis," says Sir J. J. Thomson,[3] "is that matter is just a collection of positive and negative units of electricity, and the forces which hold atoms and molecules together, the properties which differentiate one kind of matter from another, all have their origin in the electrical forces exerted by positive and negative units of electricity, grouped together in different ways in the atoms of the different elements." This amounts to an electro-magnetic theory of matter, of electrical forces between material particles. A system that admits of quantitative statement and analysis is essential to a working hypothesis. In a qualitative sense, too, if the energy of affinity, which individualizes substances, be the kinetic grouping of the elements of bodies, foci of force and centers or nuclei of condensation bear a dynamic relationship in some conceptual scale of classification, which may be called an isochrestic scale.

[3] Presidential Address: British Association, 1909. His electrical methods of experimenting tend to confirm the view that every atom (like the ultimate organic cell of the biologist) consists of an inner core or nucleus of definite significance and an outer husk of indefinite organization.

Furthermore, in the same address, Sir J. J. Thomson made the following illuminating pronouncement. "If," he said, "we regard potential energy as the kinetic energy of portions of the ether attached to the system, then all energy is kinetic energy, due to the motions of matter or of portions of ether attached to the matter....and that the potential energy of the visible universe may in reality be the kinetic energy of an invisible one connected up with it. We naturally suppose that this invisible universe is the luminiferous ether, that portions of the ether in rapid motion are connected with the visible systems, and that their kinetic energy is the potential energy of the systems."

In other words, we presume, if the ethersphere be the dynamic plenum of substance and the substratum of matter, all motion is primarily ether-motion, and all mass is essentially ether-mass, fundamentally and phenomenally: indeed, it must be the *primum mobile* of the forces of nature. The specks of matter called worlds, immersed in the maelstrom of this mighty mobile force, in silent strife of motion, and correlated streams and drifts (witness, the stars) of motion, are as jelly-fish in the ocean of space, and can no more set up independent motions of translation in respect of cosmic environment than submerged bodies in terrestrial ocean-drifts. Thus: the apparent mechanism of the universe may be likened to a seething whirlpool of interrelated currents and eddies of gyroscopic motion (or moving masses of equilibria) in which collocations of substance and aggregations of matter, at every stage of combination and in every degree of cohesion, move and have their being. And all this takes place simply by energy of relative position (i. e., difference of potential) spontaneously working on inertia of mass, until equilibrium is established, *pari passu,* within the system (i. e., equilibrium of forces between pairs of opposite sign).

This monistic world-conception may be developed into

a dynamical theory of motion—a world of moving forces, and not a world *at rest*—by assuming stability of motion to connote the moment of momentum (or static equilibrium) of a system, and to correspond with reciprocity of force in mechanical philosophy. To the dynamic mind, motion that ceases to be mobile, in the routine of cosmic phenomena, becomes mass (the product of correlated motions); and energy that ceases to be active—i. e., kinetic—becomes potential energy, available power, or force. All mass movements, as well as intermolecular action and reaction, may be expressed in terms of energy, power, or force, each of which in its ultimate expression may be held to comprise a synthesis of syntheses of motion; and unity of world-conception may be readily reached by regarding motion as the circulation of energy-systems in the ether of space, arising from difference of potential (expressed in ratios of equilibria) and giving rise, as in electricity, to associated phenomena. Thus, it seems reasonable to infer, from these generalizations, the existence of an underlying principle—the prime integral of all evolutionary processes—which, if not force, or the persistence of force, at least involves the conservation of energy. We throw out this crude suggestion as an embryo formula: That the negative instability of any system whatsoever is controlled by a positive stability of type, or *persistence of ideal configurations,* in cosmical evolution; and that all modes of matter in organic affinity are determined by typical groupings of positive and negative units of "electricity," or aggregates of particles, in specific association with homogeneous elements and heterogeneous compounds, or aggregations of mass, in the evolution of corpuscular substance. Density and mass are the watchwords of life and movement, and order of mass is the order of evolution. Thus, identity-in-difference in the microcosm may be held to connote difference of potential in the macrocosm.

Mr. F. Soddy, discussing the evolution of the elements, says: "The energy changes accompanying any form of continuous material evolution must be the controlling factor of any cosmical scheme, and the failure to recognize this so deeply affects all the existing views on cosmical and terrestrial evolution that it is difficult to see what will remain when it is rectified."—(*British Association Report*, 1906.) The trouble with schoolmen is that they cannot, or will not, think in categories of thought other than those in which they have been trained. To them, the mere attempt were like writing with the left hand.[4] Experimental science, not being able to simulate nature, necessarily has recourse to conventional methods of demonstration—methods, no doubt, that yield true results in quantitative analysis. Categories that are rigidly segregated in the lecture room are, however, intimately associated in natural processes, and these are the only real categories of genetic logic. In order to demonstrate the evolutionary movement—which of necessity must be continuous, whether we perceive the continuity or do not—the intellect must bridge the gap between the organic and inorganic realms of organization by postulating a constant of function in the universe of substance inseparably associated with a logical extension of principle in the realm of mind. Is such an aid to clear and consecutive thinking to be discovered in *verae causae*? Our thoughts, based on the significs of science and rigidly controlled by logical method, must range through the debating grounds of psychology, psychophysics, and metaphysics, to a new category of thought expressed in terms of energetics. We may then discover

[4] The Newtonian laws of motion, for instance, cannot be taken as representing absolute truth. In infinite space, and without a frame of reference, a body cannot be said to be at rest or in motion—far less, motion in a straight line. A frame of reference is essential. These laws are merely projections of our perceptual experience. If our perceptions were different—as, indeed, they are in the dynamic mind—the laws would be differently expressed. Identity-in-difference reconciles the duality.

a universal calculus, or constant of function, applicable to all forms or modes of motion. In fine, a high order of mind might derive a higher—i. e., more extended and complex—synthesis. "May we not say," remarks George Henry Lewes, "that every special form of existence, organic or inorganic, is determined by the synthesis of its elements?" That being so, a synthesis of syntheses of syntheses may be reached.

All thinking is relative. Our thoughts must have some background or framework, our mind some standard of value, before we can identify and classify any object of knowledge. To name a thing is not to create it, but to identify it; and this is the task of science. Consequently, nomenclature plays an intrinsic part in the discussion of phenomena. It is the *identity-in-difference* that characterizes objects of knowledge, rather than their abstract qualities apart from a frame of reference. Abstract and concrete ideas of things arise from introspective and extrospective analysis; and no mind can form a true synthesis of any apparent unity without complete knowledge of its constituent parts or elements. When such knowledge is incomplete, the synthesis that presumes to be called a law of nature is no more valid than any other hypothesis by which the pathway to reality may be profitably explored. Thus, all the so-called laws of nature simply represent for us a universal consensus of expert opinion, an economy of thought, a shorthand method of standardization to facilitate further inquiry. They are true, but only relatively to existing criteria and forms of knowledge. Nature knows no law, as apprehended by our anthropomorphic thought, but the law of being: *Whatever is, is,* which is the fundamental law of thought. We must take things *as given,* and try to understand them. We must think cosmically and humbly.

The advance of knowledge waits on the discovery of

mechanical media or experimental research to stimulate or augment our sense-perceptions; and mental presentations rest, in the meantime, on rational hypotheses that co-ordinate and interpret the empirical dogmas of science. Scientific faith and imagination are essential handmaids to discovery. Scientific instinct is the herald of exact knowledge. There is nothing new under the sun save in our perception of it, when it is added to the common pabulum. It is in the analogies, the correspondencies, and the homologies of nature—dangerous as these are as guides—that the man of science, by his laboratory experiments and tests, seeks for new light on old problems. Thus, the secret of electricity, which may determine our knowledge of the structure of matter and much else besides, is now being unraveled in the vacuum tube. The atom (revised version) is the philosopher's stone, which, peradventure, may carry us a little further than Plato and Aristotle.

The mere fact that coordination of phenomena and synthesis of noumena are sought in all departments of earth-lore in relation to the cosmos indicates the demand of reason for causality, since the laws of thought and the laws of motion are intimately associated in function. Unless nature[5] thought (i. e., functioned) consecutively, logically, and inevitably, it would be impossible for the man of science to formulate any rational hypothesis. But since sense-impressions and secret cosmic processes may be incongruent, or even incompatible, allies, we fall back on real logic, as being the surest path to essential truth, intuitively apprehended as such. Logical inference (no less than mathematical formulas) is an economy of thought, the shorthand of mind—that is, the trained mind or intellect, which reflects, however dimly and inscrutably, the

[5] Nature, regarded as cosmic process, is scarcely to be distinguished from the Absolute of philosophers, but is more anthropomorphic in form, and to that extent better fitted to express the whole of conceptual analysis, in cosmic function. The Absolute is revealed in the unity of nature.

mind and meaning of the cosmos. Sharper definition of focus resolves the nebulous haze that screens cosmic phenomena. If there be persistence and equivalence of mass, there must be also persistence and equivalence in the mechanical value of energy—*free* energy, determining all phenomena—in a quantitative sense, the change in consistency or valency yielding a qualitative process of evolution; and therefore it were illogical to limit evolution to ponderable substance. The visible universe is finite, simply tending to aggregation of mass; the invisible universe underlies all phenomena, of which it is the nervous system—so to speak.

Science performs a useful function in the standardization of thought, of sense-data; but it never can usurp the prerogative of mind to pronounce final judgments, nor does it advance such a claim. Science is method—a means, not an end. The most advanced thought of the day is, we may infer, endeavoring to bridge the abyss between the organic and the inorganic realms of organization — an abyss in our *knowledge* only, since evolution must deny itself if it admit discontinuity of its processes. By a secret process of integration, nature *informs* matter, and her purpose is thus satisfied, by specific molecular association of individualized substances. "The true cosmic line," says R. B. Arnold,[6] "in the matter of individuation, is—the electron, corpuscle, atom, molecule; then, perhaps, the physiological unit in the vital organism, and finally the mental being." This line of attack is reinforced by Mendelian method, which is the idea of a living organism as built up of numerous unit-characters represented in the germ-cell by definite factors. Such a scheme were capable of quantitative, as well as qualitative, analysis; i. e., constant ratio of quantity, as well as constancy of association, in nature's laboratory. Hence, in the phenomena of life

[6] *Scientific Fact and Metaphysical Reality*, pp. 104-5.

and mind, ratios of disparities—if such can be measured —would determine, by difference of potentiality in related categories, the movement of thought—i. e., development of idea—in substance. The persistence of psychic life thus may be traced, from its genesis in self-consciousness—"I am" — to its penultimate issue, in the realm of mind. Whether thought can be carried further than organized inclusive experience—i. e., mind—is the "riddle of the universe," as we understand it. That depends on experience; and the world is still young. "Men should work, as if they were to live for ever; and live, as if they had to die to morrow." The work is everything, the man is nothing—to posterity.

Finally, our concepts of time and space in terms of formal thought may be delivered thus: Time and space may be defined as sense-intuition of that which is internal and external, respectively, to mind, while as a composite reality and essential unity these prime concepts, in our consciousness of being, are phenomenal (*qua* sense-intuition) in the inherent condition of any connotative or genetic relationship. In all interactions between time and space, the ratio of relativity remains unaltered. All depends upon the point of view, and until this fundamental principle in cosmic evolution is grasped and applied, professors of your Know-not-what school will vex their souls, and their students, in vain. Thus, ether may be continuous as regards matter and discontinuous as regards substance (in the degradation of energy) without losing its essential character of continuity in substance, any change of relative consistency, or valency, being due to difference of potential, as in the flow of electricity from one body to another. The ether must, in fact, be cleavable—though continuous in respect of "gross" matter—in the sense that water and air admit of differential movements of equilibria. Given an ether of the density claimed for it, we can cenceptualize

a cosmos *congruous with known truths.* The ether of space, in which the warfare of worlds is waged with as little commotion to its mass as from the dropping of a pebble into the sea, which bears the stress of the universe of matter as easily as a radiogram from America to Europe, contains for us, it may be, the arcana of nature. It is the last unexplored region in the realm of mind which man may aspire to reach. It is a fruitful field for the philosopher of nature, in which the psychologist may find his *psyche* and the biologist his *life.*

We argue in this way. If, as a necessity (or even as a postulate) of reason, a missing article (or connecting link) *must be* located in one of twelve places, and we ransack eleven of these places without finding it, then *it must be* in the twelfth place, even, though that place be beyond our reach and therefore excluded from actual investigation. This conviction of mind produces faith in the understanding. We adopt the view of Mr. Balfour, that as natural science grows it leans more, not less, upon an idealistic interpretation of the universe." The conservation of thought-values is the sole objective of truth. The known and the unknown, the natural and the supernatural, are correlated terms. They complement one another, in the ratio that decrease of the one demands increase of the other: the momentum of truth. Intellectual life is a great conversation, having for one of its objects the elucidation of phenomena. Progress is like a swallow's flight—a dip and flow along the mean level of current thought.

If a complete synthesis or sequence of cosmic phenomena could be thrown on a screen by cinema-process, the various phases of evolution would blend into a perfect unity and function harmoniously; but, examined separately, each stage would be finite and imperfect. It is by intuitional perception of the principle of continuity in the logic of nature that we connect the invisible with the vis-

ible systems in the correlation of phenomena, both in the organic and inorganic realms of organization. Our fundamental assumption is, that the law and order which we perceive in nature necessarily imply and denote intelligent purpose; and life and mind define that purpose as the evolution of spiritual being, thereby indicating and postulating the persistence of psychic life beyond the present range of human intellection. By psychic selection, mind must dominate body in an ever-increasing degree, to which no limit can be set. If the purely utilitarian, determinist, and mechanical aspects of evolution be insisted upon, it is impossible to account (and we need not apologize) for the remarkable development of psychic function, which compels man to subordinate his material advancement to the dictates of his conscience and his sense of duty. Self-sacrifice and not self-preservation is characteristic of those whom we honor and love; and it is in the equitable or rational adjustment between egoism and altruism that we recognize the sanity and virility of the race. Race-instinct cannot be divorced from the persistent force which creates heroes out of men, martyrs out of saints, and seers out of thinkers. *Ideas rule the world.* What will people, ten thousand years hence, think of our philosophy? Surely, we must make provision for some intelligent development in the expansion of thought. It takes much weight of wisdom to plumb the depth of our ignorance—the greater the wisdom, the greater the depth—but shallow fools reach mud with surprising ease. The revolt of man against spiritual extinction is a tribute to his intelligence, which demands a *meaning* for life and mind, other than the reproduction of species.

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THE PRINCIPLES OF MECHANICS WITH NEWTON FROM 1679 TO 1687.*

I.

MANY times on the subject of Newton's optical publications, Hooke had claimed priority, and there was in consequence some friction between Hooke and Newton. That such disputes were agreeable neither to Hooke nor to Newton appears from letters which passed between them in 1676.[1]

It is probable that the intermeddling friend, referred to by Hooke, who stirred up strife between them by painting Hooke in dark colors to Newton was Henry Oldenburg, the secretary of the Royal Society.[2] In the disputes between Hooke and Hevelius on the subject of plain and telescopic sights, and between Hooke and Huygens about the invention of pendulum clocks, Oldenburg took a keen and active part against Hooke. However, in a later dispute of 1686, and therefore long after Oldenburg's death, Halley remarked to Newton that he feared that Hooke's manner of claiming the discovery of the law of the inverse squares had been "represented in worse colors than it ought."[3]

* The references are mainly to the works mentioned in my previous article in this journal (April, 1914) on "The Principles of Mechanics with Newton up to 1679."

[1] Brewster, *op, cit.*, pp. 140-143.

[2] *Ibid.*, pp. 143-144.

[3] Rouse Ball, *op. cit.*, p. 163; Brewster, *op cit.*, p. 144.

UMI

We shall now have to consider some correspondence that passed between Hooke and Newton in 1679 and 1680, which is very important, since it was the means of turning Newton's thoughts again to the subject of gravitation. It also forms the ground-work of the claim put forward later by Hooke to the discovery of the Newtonian theory of gravitation.

The "Royal Society for the improving of Natural Knowledge" was founded in 1662.[4] As mentioned in my article on "Robert Hooke as a Precursor of Newton,"[5] Hooke was its first "curator of experiments" and Henry Oldenburg its first secretary. When[6] Oldenburg died in 1677 Hooke succeeded him as secretary, and wrote to Newton on November 24, 1679, as follows:[7]

"Sir,

Finding by our registers that you were pledged to correspond with Mr. Oldenburg, and having also had the happiness of receiving some letters from you myself, make me presume to trouble you with this present scribble—Dr. Grew's more urgent occasions having made him decline the holding correspondence. And the Society has devolved it on me. I hope, therefore, that you will please to continue your former favors to the Society by communicating what shall occur to you that is philosophical, and, for return, I shall be sure to acquaint you with what we shall receive considerable from other parts, or find out new here. And you may be assured that whatever shall be so communicated shall be no otherwise further imparted or disposed of than you yourself shall prescribe. I am not ignorant that both heretofore, and not long since also, there have been some who have endeavored to misrepresent me to you, and possibly they or others have not been wanting

[4] It celebrated its 250th anniversary in July, 1912.
[5] *Monist*, July, 1913, § 1.
[6] Cf. *ibid.*, § VII.
[7] Rouse Ball, *op. cit.*, pp. 139-140.

[lacking] to do the like to me; but difference in opinion, if such there be, should not, especially in philosophical matters where interest hath little concern, methinks, be the occasion of enmity—it is not with me, I am sure. For my part I shall take it as a great favor if you shall please to communicate by letter your objections against any hypothesis or opinion of mine; and particularly if you will let me know your thoughts of that of compounding the celestial motions of the planets out of a direct motion by the tangent and an attractive motion towards the central body, or what objections you have against my hypothesis of the laws or causes of springiness."

Thus began the first letter of a correspondence[8] through which Newton was, as Pemberton says, led to inquire what was the real figure in which a body, let fall from any high place, descends, taking the motion of the earth round its axis into consideration; and, as Newton acknowledged in a letter[9] written in 1686 to Halley:

"This is true, that his [Hooke's] letters occasioned my finding the method of determining figures, which, when I had tried in the ellipsis, I threw the calculations by, being upon other studies; and so it rested for about five years, till upon your request I sought for that paper; and not finding it, did it again, and reduced it into the propositions shown you by Mr. Paget: but, for the duplicate proportion,

[8] Brewster, *op. cit.*, Vol. I, pp. 291-292, 254-255; Rouse Ball, *op. cit.*, pp. 18-24, 139-150, 155-167. It is very likely that Newton had his attention first directed to Picard's measurement by Hooke (see below), and Brewster (*op. cit.*, pp. 291-292; cf. Rouse Ball, *op. cit.*, pp. 22-24) seems wrong in stating that Newton did not resume his former thoughts concerning the moon till 1684 (cf. Pemberton in Rouse Ball, *op. cit.*, p. 20). It is amusing to compare Conduitt's (Brewster, *op. cit.*, pp. 291-292) version of the correspondence with Hooke with the actual letters; for Conduitt stated that he got his information from Newton. Newton, in his first letter to Hooke, explicitly said "spiral line"; Conduitt attributed the error to a "negligent stroke of the pen" in the diagram. Newton himself wrote to Halley on May 27, 1686: "In the scheme wherein I explained this, I carelessly described the descent of the falling body in a spiral to the center of the earth: which is true in a resisting medium, such as our air is." Cox (*op. cit.*, p. 93) is mistaken in saying that Newton worked out the details of his discovery from 1672 (when Picard communicated his results to the Royal Society) to 1674.

[9] Rouse Ball, *op. cit.*, p. 165.

I can affirm that I gathered it from Kepler's theorem about twenty years ago."

Again, in a letter written to Halley thirteen days afterwards, Newton said:[10]

"And, though his correcting my spiral occasioned my finding the theorem by which I afterwards examined the ellipsis, yet am I not beholden to him for any light into the business, but only for the diversion he gave me from my other studies to think on these things, and for his dogmaticalness in writing as if he had found the motion in the ellipsis, which inclined me to try it, after I saw by what method it was to be done."

For this reason we will here consider the correspondence in some detail.

In the first letter Hooke, after the opening paragraph quoted above, mentioned two things. Firstly, he desired to know Newton's opinion of a hypothesis of the heavens, invented by Mallemont de Messanges;[11] and, secondly he mentioned that Picard and others were making geodetical observations in France.[12] "I am informed, likewise from Paris, that they are there about another work, viz., of settling the longitude and latitude of the most considerable places: the former of those by the eclipses of the satellites of Jupiter. MM. Picart and De la Hire travel, and MM. Cassini and Romer observe at Paris. They have already found that Brest in Brittany is eighteen leagues nearer Paris than all the maps make it. I have written to a correspondent in Devonshire to see if he can do somewhat of that kind here, and I should be glad if, by perpendicular observations, we could determine the difference of latitude between London and Cambridge. If you know of any one that will observe at Cambridge, I will procure it to be done here very exactly."

[10] *Ibid.*, 167.
[11] On this man, see Rigaud, *op. cit.*, pp. 42-43.
[12] Rouse Ball, *op. cit.*, p. 140.

Hooke finally gave some small items of news about a book by De la Hire and some observations of Flamsteed's.

This letter was copied by Rouse Ball from a rough draft in Hooke's handwriting which is in the Library of Trinity College, Cambridge;[13] and it must be remembered when reading Rigaud's *Essay*,[14] that Rigaud had not seen this correspondence.

It was probably this letter which led Newton to use Picard's estimate in the revision of his calculation.[15]

Newton replied on Nov. 28, 1679,[16] and his letter began:

"I cannot but acknowledge myself every way by the kindness of your letter tempted to concur with your desires in a philosophical correspondence. And heartily sorry I am that I am at present unfurnished with matter answerable to your expectations—for I have been this last half year in Lincolnshire cumbered with concerns amongst my relations till yesterday when I returned hither; so that I have had no time to entertain philosophical meditations, or so much as to study or mind anything else but country affairs. And before that, I had for some years last been endeavoring to bend myself from philosophy to other studies, in so much that I have long grudged the time spent in that study unless it be perhaps at idle hours sometimes for a diversion; which makes me almost wholly unacquainted with what philosophers at London or abroad have of late been employed about. And perhaps you will incline the more to believe me when I tell you that I did not, before the receipt of your last letter, so much as hear (that I remember) of your hypothesis of compounding the celestial motions of the planets out of a direct motion by

[13] *Ibid.*, p. 139.

[14] Cf. pp. 12, 37, 42.

[15] Rouse Ball, *op. cit.*, p. 23; cf. the quotation of Pemberton's confirmatory evidence and Robison's—probably mistaken—account on pp. 20-23. Cf. Brewster, *op. cit.*, 292.

[16] Rouse Ball, *op. ct.*, pp. 141-144.

the tangent to the curve, and of the laws and causes of springiness, though these no doubt are well known to the philosophical world. And having thus shook hands with philosophy and being also at present taken up with other business, I hope it will not be interpreted out of any unkindness to you or the Royal Society that I am backward in engaging myself in these matters, though formerly I must acknowledge I was moved by other reasons to decline, as much as Mr. Oldenburg's importunity and ways to engage me in disputes would permit, all correspondence with him about them. However I cannot but return my hearty thanks for your thinking me worthy of so noble a commerce and, in order thereto, frankly imparting to me several things in your letter."

In a letter of June 20, 1686, to Halley, Newton thus[17] described this correspondence:

"....In my answer to his first letter I refused his correspondence, told him I had laid philosophy aside, sent him only the experiment of projectiles (rather shortly hinted than carefully described), in compliment to sweeten my answer, expected to hear no further from him; could scarce persuade myself to answer his second letter; did not answer his third,—was upon other things; thought no further of philosophical matters than his letters put me upon it, and therefore may be allowed not to have had my thoughts of that kind about me so well at that time."

Indeed, Newton, after briefly discussing Mallemont's hypothesis, and after referring to a possible determination of latitude and to Flamsteed's observations,[18] proceeded[19]:

"In requital of this advertisement, I shall communicate to you a fancy of my own about discovering the earth's diurnal motion. In order thereto, I will consider the earth's diurnal motion alone, without the annual, that hav-

[17] *Ibid.*, p. 157.
[18] *Ibid.*, p. 142.
[19] *Ibid.*, pp. 142-143.

ing little influence on the experiment I shall here propound. Suppose then BDG represents the globe of the earth carried round once a day about its center C from west to east, according to the order of the letters BDG; and let A be a heavy body suspended in the air, and moving round with the earth so as perpetually to hang over the same point thereof, B. Then imagine this body A let fall; its gravity will give it a new motion towards the center of the earth without diminishing the old one from west to east. Whence the motion of this body from west to east, by reason that before it fell it was more distant from the center of the earth than the parts of the earth at which it arrives in its fall, will be greater than the motion from west to east of the parts of the earth at which the body arrives in its fall; and therefore the body will not descend the perpendicular AC, but, outrunning the parts of the earth, will shoot forward to the east side of the perpendicular, describing in its fall a spiral line ADEC, quite contrary to the opinion of the vulgar who think that, if the earth moved, heavy bodies in falling would be outrun by its parts and fall on the west side of the perpendicular. The advance of the body from the perpendicular eastward will, in a descent of but twenty or thirty yards, be very small, and and yet I am apt to think it may be enough to determine the matter of fact. Suppose, then, in a very calm day, a pistol bullet were let down by a silk line from the top of a high building or well, the line going through a small hole made in a plate of brass or tin fastened to the top of a building or well; and that the bullet when let down almost to the bottom were settled in water so as to cease from swinging, and then let down further on an edge of steel lying north and south to try if the bullet in settling thereon will almost stand *in aequilibrio* but yet with some small propensity (the smaller the better) decline to the west side of the steel as often as it is so let down thereon. The steel being so placed underneath,

suppose the bullet be then drawn up to the top and let fall by cutting, clipping, or burning the line of silk. If it fall constantly on the east side of the steel, it will argue the diurnal motion of the earth. But what the event will be I know not, having never attempted to try it. If anybody would think this worth their trial, the best way, in my opinion, would be to try it in a high church or wide steeple, the windows being first well stopped; for, in a narrow well, the bullet possibly may be apt to receive a ply from the straitened air near the sides of the well, if in its fall it come nearer to one side than to another. It would be convenient also that the water into which the bullet falls be a yard or two deep or more, partly that the bullet may fall more gently on the steel, partly that the motion which it has from west to east at its entry into the water may, by means of the longer time of descent through the water, carry it on further eastward and so make the experiment more manifest."

In the figure which accompanied this letter, the path of the falling body from A to D was indicated by a dotted line which was continued into the interior of the earth, and, after having turned once round the center C, ended at C. This is the spiral often referred to below, as well as by Newton in a note above.

This letter was read to the Royal Society on Dec. 4, 1679, as appears by the following minute:[20]

"Mr. Hooke produced and read a letter of Mr. Newton to himself dated 28th November, 1679, containing his sentiments of Mallemont's new hypothesis of the heavens; and also suggesting an experiment whereby to try whether the earth moves with a diurnal motion or not, viz., by the falling of a body from a considerable height, which, he alleged, must fall to the eastward of the perpendicular, if the earth moved. This proposal of Mr. Newton's was highly ap-

[20] Birch, *op. cit.*, Vol. III, 1757, pp. 512-513; Rouse Ball, *op. cit.*, p. 145.

proved of by the Society; and it was desired that it might be tried as soon as could be with convenience. Sir Christopher Wren supposed that there might be something of this kind tried by shooting a bullet upwards at a certain angle from the perpendicular round every way, thereby to see whether the bullets so shot would all fall in a perfect circle round the place where the barrel was placed. This barrel he desired might be fixed in a frame upon a plane foot, and that foot placed upon a true plane every way, and the mouth of the gun be almost in the same point over the plane which way soever shot. Mr. Flamsteed hereupon alleged that it was an observation of the gunners that, to make a ball fall back into the mouth of the piece, it must be shot at eighty-seven degrees; and that he knew the reason thereof; and that it agreed with his theory; and that a ball shot perpendicularly would never fall perpendicularly; and he mentioned the recoiling of a perpendicular jet of water. But this was conceived to arise from some mistake of the gunners in not well taking notice of all circumstances; since a body shot perpendicularly would also descend perpendicularly; and a body shot at eighty-seven degrees would fall considerably distant from the place where it was shot."

Newton's letter then ended:[21]

"If I were not so unhappy as to be unacquainted with your hypothesis above mentioned (as I am with almost all things which have of late been done or attempted in philosophy), I should so far comply with your desire as to send you what objections I could think of against them, if I could think of any. And, on the other hand, I could with pleasure hear and answer any objections made against any notions of mine in a transient discourse for a divertisement. But yet, my affection to philosophy being worn out, so that I am almost as little concerned about it as one

[21] Rouse Ball, *op. cit.*, p. 144.

tradesman uses to be about another man's trade or a country man about learning, I must acknowledge myself averse from spending that time in writing about it which I think I can spend otherwise more to my own content and the good of others; and I hope neither you nor anybody else will blame me for this averseness. To let you see that it is not out of any shyness, reservedness, or distrust that I have of late and still do decline philosophical commerce, but only out of my applying myself to other things, I have communicated to you the notion above set down (such as it is) concerning the descent of heavy bodies for proving the motion of the earth; and shall be as ready to communicate in oral discourse anything I know, if it shall ever be my happiness to have familiar converse frequently with you. And possibly if anything useful to mankind occurs to me I may sometimes impart it to you by letter."

Hooke replied on Dec. 9, 1679, by a letter which has been lost. A note, however, written by Hooke, contains the words:[22]

"Dec. 9th, 1679, in a letter to Newton, Hooke has these words upon an account of an experiment about the falling of a ball from a considerable height: I could add many other considerations consonant to my theory of circular motions compounded by a direct motion and an attractive one to the center, etc."

It was read to the Royal Society on Dec. 11, 1679:[23]

"Upon the mentioning of Mr. Newton's letter and the experiment proposed in it, Mr. Hooke read his answer to him upon that subject, wherein he explained what the line described by a falling body must be supposed to be, moved

[22] *Ibid.*, pp. 152-153.

[23] Birch, *op. cit.*, Vol. III, p. 516; Rouse Ball, *op. cit.*, p. 146; Rigaud, *op. cit.*, pp. 12-13. This extract has already been given, in rather different words of Waller's, in § VII of my article on Hooke (*Monist*, July, 1913).

circularly by the diurnal motion of the earth and perpendicularly by the power of gravity: and he showed that it would not be a spiral line, as Mr. Newton seemed to suppose, but an excentrical elliptoid [*sic*], supposing no resistance in the medium: but, supposing a resistance, it would be an excentric ellipti-spiral, which, after many revolutions, would rest at last in the center; that the fall of the heavy body would not be directly east, as Mr. Newton supposed, but to the south-east, and more to the south than the east. It was desired that what was tryable in this experiment might be done with the first opportunity."

Besides this, Rigaud[24] wrote:

"On the 11th of Dec., 1679,[25] he [Hooke] had explained that the body in its fall would be carried towards the south as well as towards the east, and this he described as having occurred in actual trial; but at the same time it is to be acknowledged that the coincidence must have been the consequence of accidental circumstances, since the fall, being only 27 feet, was too small to produce a deviation which he could have appreciated."

With regard to the further history of Newton's problem, we may remark, after Rigaud,[26] that it was treated by Thomas Simpson[27] and d'Alembert.[28] The experiment has also been tried with success by Gulielmini, Henzenberg, and Flaugergues, from stations of very great height; but still there are so many difficulties attending it that Delambre[29] considered it as by no means conclusive. It was found, indeed, by mathematicians that the deviation to the south is exceedingly small for any case in which it can be actually tried.

[24] *Op. cit.*, p. 44.
[25] Birch, *op. cit.*, Vol. III, p. 11.
[26] *Op. cit.*, pp. 44-45.
[27] *Mathematical Dissertations*, 1743, p. 160.
[28] *Histoire de l'Académie de Paris*, 1771, p. 10.
[29] *Astronomie*, Vol. II, p. 192.

II

We now come to the question of the spiral.[1] If the earth were at rest, a heavy body would fall in a straight line towards its center, but if the motion communicated by the rotation acts as a projectile force, the combination of it with gravity would, Newton at first hastily stated, make the body descend in a spiral. Biot[2] said that doubtless Newton came to this conclusion because he supposed the fall to be in a resisting medium like the air, and this, at first sight, seems very probable, especially when we consider what is laid down in the fourth section of the second book of the *Principia*. But, said Rigaud, Newton said nothing to confirm this conjecture[3]: on the contrary, he rested his defence upon being engaged on other pursuits, so that "I thought no further of philosophical matters than his letters put me upon it, and therefore may be allowed not to have had my thoughts of that kind about me so well at that time." Indeed the proposition was probably not communicated as the result of his own speculations, but as it casually occurred to his recollection in the act of writing. He had not then demonstrated the law of central forces, and the early impression of their effect seems very generally to have been that revolving bodies might ultimately fall down to the point to which they were attracted. This naturally led to the idea of a spiral path. Leonardo da Vinci had assigned it to them, and it had been maintained nearer to Newton's own time and under circumstances which were more likely to be present to his thoughts. There had been a controversy, in which the arguments derived by Riccioli from falling bodies against the earth's motion were answered by Stephanus de Angelis. An account of what passed

[1] Rigaud, *op. cit.*, pp. 45-47.

[2] In his biography of Newton in the *Biographie Universelle*, Vol. XXXI, p. 149; Eng. Trans., p. 14.

[3] Rigaud had not seen Newton's letter to Halley of May 27, 1686, mentioned in one of the preceding notes and confirming Biot's conjecture.

between them was drawn up by James Gregory, and published in the second volume of the *Philosophical Transactions*. Mention was made, at the end, of the experiment tried by Descartes of discharging cannon balls perpendicularly in the air, and of their falling back to the surface more frequently to the west than to the east. This experiment, possibly too difficult of accurate execution to be at all relied upon, was brought forward in confirmation of the earth's motion, and it may have suggested that which Newton recommended for the same purpose. As De Angelis had argued against the false position of his antagonist, his views may have been received with the more favor. Now he assumed the spiral motion, although it is not noticed by Gregory, as we see by the preface to his tract *De infinitis spiralibus inversis*.[4]

Among David Gregory's papers there is, said Rigaud,[5] a memorandum that "Mr. Newton pursued the motion of the planets first on supposition of an equal gravity, but afterwards wrote to Mr. Hooke, asking what he suspected the law of gravity to be." That Newton, before 1666, may at first have entertained such an idea of gravitation as is here described is very possible; but it is most evident, from Hooke's own statement, that his letter in 1679 originated with himself, and was no reply to any application which Newton had made to him.

III.

The line of argument which Newton adopted in this correspondence with Hooke supplies, said Rigaud,[1] a con-

[4] Pat. 1667: "Jam anni quadrans evolvit, ex quo discursus quidam nostri Italice conscripti prodiere; in quibus rationes aliquot physico-mathematicas contra Coperniceum systema a viro eximio Ricciolio excogitatas, ad trutinam revocavimus. Affirmabamus tunc bina genera infinitarum spiralium a nobis fuisse contemplata; spatiaque ab ipsis clausa geometrice mensurata; quarum una illa foret, quae a gravi naturaliter cadente in plano aequatoris describeretur, is Tellus, ex falsa hypothesi, motu diurno duntaxat moveretur, et grave deorsum latum taliter suum concitaret motum, ut spatia, ab ipso peracta, forent ad invicem ut ipsa quadrata temporum." See also p. 17, and *Cartesii Epist.*, Part. prim., pp. 162, 173.

[5] *Op. cit.*, p. 47. [1] *Op. cit.*, pp. 48, viii.

firmation of his not having yet made use of Picard's measures for the correction of his first calculations. If he had himself "experimentally verified" his hypothesis, he would have hardly failed to mention it when he repeatedly pointed out Hooke's deficiency in this respect; and he distinctly said that, "before a certain demonstration" which he found in 1685, he had suspected that the law of the inverse squares "did not reach accurately enough down so low" as "the superficies of the earth," "and therefore in the doctrine of projectiles never used it nor considered the motion of the heavens." However this may be, he clearly followed his own precise rule of philosophizing: "In experimental philosophy we are to look upon propositions collected by general induction from phenomena as accurately or very nearly true, notwithstanding any contrary hypotheses that may be imagined, till such time as other phenomena occur by which they may either be made accurate or liable to exceptions."

Hooke's process, said Rigaud,[2] was much less strict; and, although it enabled him to take one great step towards the truth, it did not put him in possession of it either completely or with certainty. He saw the analogy between the moon's motion and that of the primary planets, and accordingly extended the law of the inverse squares to terrestrial gravitation, without knowing that this law of the effect ceased at the surface of the earth. He had left the verification of his theory to his readers in 1674, which made Newton very justly complain[3] that he should claim the honor of discovery for himself, and throw upon others the drudgery of ascertaining whether after all it was applicable to the facts. It is probable that he had only adopted the precise law of variation when he wrote to Newton in 1679. Hooke satisfied himself "that the proportion was

[2] *Op. cit.*, pp. 49-50, viii.
[3] Letter to Halley of June 20, 1686; Rouse Ball, *op. cit.*, p. 159.

duplicate *quam proxime* at great distances from the center, and only guessed it to be so accurately,"[4] but without Newton's demonstrations, to which he was a stranger, he had advanced no further than to an imperfect though beautiful conjecture. It may likewise be added that he had not that clear and steady view of his own first principles which, if he had attempted it, would have enabled him to arrive at more certain conclusions. In a lecture which he delivered as late as the 25th of May, 1687, he said: "I know that if the gravitating power in the sun and moon be exactly the same with that of the earth, the query I propounded can have no ground; but, though they may in most particulars be consonant, as I shall prove in my theory of gravity, yet there may be a cause (and there seems to be some assignable) why there may be something specific in each of them."

Arguing from the analogies which he had adopted, Hooke rejected Newton's suggestion of a spiral motion for the projectile, and said that its path would be an elliptical curve, but he did not venture the assertion of its being an ellipse: he only called it an eccentrical ellipsoid; and added that, supposing the motion to take place in a resisting medium, the path would become an "eccentric ellipti-spiral."

IV.

Hooke certainly saw that the earth must be oblate at the poles; mention was made of this theory in many parts of his writings, and particularly in his *"Discourse on Earthquakes,"* which Waller, the editor of his *Posthumous Works,* considered to have been written before 1667. He considered the solid part of the earth to be an exterior shell, and reasoned as to the figure which it assumed, from the different degrees of centrifugal force of the parts at

[4] *Ibid.*, p. 158.

different distances from the axis of rotation. Now this was a fact of which Newton did not need to be informed, for, in comparing the moon's orbit in 1666 with the effect of gravity, he calculated (as he said) "the force of ascent at the equator arising from the earth's motion"; and he resolved the problem more completely than it had ever been contemplated by Hooke, who assumed as the foundation of his argument that "the gravitating power of the earth be everywhere equal, as I know no reason to suppose the contrary"; and said that the waters of the sea may, at the poles, have a plane, if not a concave surface.[1]

V.

Newton replied to Hooke's letter of Dec. 9, but the reply is lost.[1] When Hooke said that the path was not a spiral but an "eccentrical elliptoid," or as Newton put it in a letter to Halley of 1686,[2] "Mr. Hooke replied it would not descend to the center but at a certain limit return upwards again. I then took the simplest case for computation, which was that of gravity uniform in a medium not resisting, imagining he had learned the limit from some computation, and for that end had considered the simplest case first. And in this case I granted what he contended for, and stated the limit as nearly as I could." In his reply of Jan. 6, 1680,[3] Hooke began:

"Your calculation of the curve described by a body attracted by an equal power at all distances from the center, such as that of a ball rolling in an upward concave cone, is right, and the two auges will not unite by about a third of a revolution; but my supposition is that the attraction always is in duplicate proportion to the distance from the center reciprocal, and consequently that the ve-

[1] Rigaud, *op. cit.*, p. 67.
[1] Rouse Ball, *op. cit.*, pp. 146-147.
[2] *Ibid.*, p. 155.
[3] *Ibid.*, p. 147.

locity will be in a subduplicate proportion to the attraction, and consequently, as Kepler supposes, reciprocal to the distance. and that with such an attraction the auges will unite in the same part of the circle, and that the nearest point of the access to the center will be opposite to the farthest distant, which I conceive doth very intelligibly and truly make out all the appearances of the heavens. And therefore (though in truth I agree with you that the explicating the curve in which a body descending to the center of the earth would circumgyrate were a speculation of no use, yet) the finding out the properties of a curve made by two such principles will be of great concern to mankind, because the invention of the longitude by the heavens is a necessary consequence of it, for the composition of two such motions I conceive will make out that of the moon. What I mentioned in my last concerning the descent within the body of the earth was but upon the supposal of such an attraction, not that I really believe there is such an attraction to the very center of the earth, but, on the contrary, I rather conceive that the more the body approaches the center the less will it be urged by the attraction, possibly somewhat like the gravitation on a pendulum or a body moved in a concave sphere where the power continually decreases the nearer the body inclines to a horizontal motion which it has when perpendicular under the point of suspension or in the lowest point, and there the auges are almost opposite, and the nearest approach to the center is at about a quarter of a revolution. But, in the celestial motions, the sun, earth, or central body is the cause of the attraction, and, though they cannot be supposed mathematical points, yet they may be conceived as physical, and the attraction at a considerable distance may be computed according to the former proportion as from the very center. This curve truly calculated will show the error of those many lame shifts made use of by

astronomers to approach the true motions of the planets with their tables. But of this more hereafter."

Hooke then[4] gave an account of his trials of the "noble experiment" of the falling body, and added: "If it doth succeed there will follow several other consequences not less considerable—as, first, that all bodies will, in consequence, grow lighter the nearer they approach the equinoctial, the circular motion being the swifter, and for the same reason the further a body is from the center the less will be its gravitation, not only on account of the decrease of the attractive power, which I have a long time supposed, but upon the increase of the endeavor of recess. And this gives us another way to try whether the earth has a diurnal motion, though much short of what you proposed. But that I may tell you somewhat of observation, Mr. Halley, when he returned from St. Helena, told me that his pendulum at the top of the hill went slower than at the bottom, which he was much surprised at, and could not imagine a reason. But I presently told him that he had solved me a query I had long desired to be answered but wanted opportunity, and that was to know whether the gravity did actually decrease at a greater height from the center. To examine this decrease of attraction I have formerly made many experiments on Paul's steeple and Westminster Abbey, but none that were fully satisfactory. This will spoil the universal standard by the pendulum and the equality of pendulum clocks carried from one climate to another."

Newton said of this letter in 1686:[5]

"He replied that gravity was not uniform but increased in descent to the center in a reciprocal duplicate proportion of the distance from it, and thus the limit would be otherwise than I had stated it, namely, at the end of every entire revolution, and added that, according to this duplicate

[4] *Ibid.*, p. 148; cf. pp. 148-149.
[5] *Ibid.*, p. 155.

proportion, the motions of the planets might be explained and their orbs defined. This is the sum of what I remember."

Later he remembered something else:[6]

"He told me that my proposed experiment about the descent of falling bodies was not the only way to prove the motion of the earth; and so added the experiment of your pendulum clock at St. Helena as an argument of gravity's being lessened at the equator by the diurnal motion. The experiment was new to me, but not the notion; for, in that very paper which I told you was writ some time above fifteen years ago, I calculated the force of ascent at the equator, arising from the earth's diurnal motion, in order to know what would be the diminution of gravity thereby. But yet to do this business right is a thing of far greater difficulty than I was aware of."

Hooke again referred to the problems on orbits under attraction in a letter of Jan. 17, 1680,[7] where also the experiments were further referred to,[8] and then the correspondence on this subject closed.

With the controversy[9] which arose out of this correspondence, I shall not deal in any detail. The facts on which Hooke's claims were based are given above and in the following references.

With Hooke's *Attempt to prove the Annual Motion of the Earth* of 1674, which had been previously read to the Royal Society at Gresham College, I have already dealt in section V of "Robert Hooke as a Precursor of Newton."[10] His *De Cometa* of 1678 was dealt with in section VI, and

[6] *Ibid.*, p. 165.
[7] *Ibid.*, p. 149.
[8] Cf. *ibid.*, p. 150.
[9] Cf. Hooke's notes (*ibid.*, pp. 150-153, 144-145); Newton's correspondence of 1686 with Halley (*ibid.*, pp. 154-155, 156-167); *ibid.*, pp. 68-73; and Rigaud, *op. cit.*, pp. 37-71.
[10] *Monist*, July, 1913..

his early paper of 1666 and a pendulum experiment in section III.

Hooke wrote, in the letter[11] of Sept. 15, 1689, which was to pass for Aubrey's to Antony Wood, that he wrote to Newton "to make a demonstration of *this theory* [in Hooke's *Attempt*] *not* telling him *at first* the proportion of the gravity to the distance, [and] *nor what was* the curved line that was thereby made.

"Mr. Newton, in his answer to the letter, *did express* that he had not thought of it; and in his first attempt about it, he calculated the curve by supposing the attraction to be the same at all distances."

Thus Newton was led to consider what would be the form of the curve which a body projected from a point and acted upon by a central attraction varying inversely as the square of the distance would describe. Then too he established the theorem relating to the equal description of areas, and discovered the method of determining the law of central force in order that any given orbit might be described under its action. He applied this to the ellipse and proved that this curve would be described under the action of a force in the focus varying inversely as the square of the distance.[12]

VI.

According to J. W. L. Glaisher, in his address delivered on the bicentenary of the publication of the *Principia*, Newton's thoughts were, in 1666, carried from the earth to the moon. Kepler's laws had reference to the sun and planets, so we may safely assume that Newton's mind had grasped the idea of a single principle of gravitation governing the motions of bodies on earth and in the heavens.

[11] Rigaud, *op. cit.*, App. No. XIII, p. 52. Hooke's interlineations are printed in italics.

[12] This is from Glaisher's account referred to below, which is partly based on the manuscript note of Newton's reproduced above from Rouse Ball's book, pp. 6-7. Cf. Rouse Ball, *op. cit.*, pp. 20-21.

This appears to be similar to the opinion of Adams referred to above. In this view there are three distinct steps in Newton's great discovery; the first is physical and the other two are mathematical. The first is the conception, that was present to Newton's mind in 1666, that all the particles of matter in the earth and the moon attract each other according to the law of the inverse square; the second is the mathematical investigations of 1679; and the third is the theorem of 1685 on the attraction of a sphere.

By this determination of the attraction of a sphere on an external particle,[1] and by the unexpected—as Newton himself said—finding that this attraction is directed to the center of this sphere, Newton saw, as Glaisher[2] has put it, all the mechanism of the universe at once spread out before him. "When he discovered the theorems that form the first three sections of Book I, when he gave them in his lectures of 1684, he was unaware that the sun and earth exerted their attractions as if they were but points. How different must these propositions have seemed to Newton's eyes when he realized that these results, which he had believed to be only approximately true when applied to the solar system, were really exact! Hitherto they had been true only in so far as he could regard the sun as a point compared to the distance of the planets or the earth as a point compared to the distance of the moon—a distance amounting to only about sixty times the earth's radius—but now they were mathematically true, excepting only for the slight deviation from a perfectly spherical form of the sun, earth, and planets. We can imagine the effect of this sudden transition from approximation to exactitude in stimulating Newton's mind to still greater efforts. It was now in his power to apply mathematical analysis with absolute precision to the actual problems of astronomy."

[1] Rouse Ball, *op. cit.*, pp. 11, 35, 61, 157.
[2] *Ibid.*, p. 61.

The discovery of the theorem by which the attraction of a spherical mass is determined was thus rightly emphasized by Glaisher—as it was by Edleston[3] — as an immensely important—perhaps in a sense the most important —of Newton's researches on gravitation. But when Newton said, as he did in the letter of 1686 already quoted,[4] that he never "considered the motions of the heavens" before he discovered this theorem in 1685, his statement is so plainly wrong that an "it in"—where the "it" refers to the law of attraction below the surface of the attracting body—must have been omitted by mistake between the "considered" and the "the."

Hooke, in his paper read to the Royal Society on March 21, 1666,[5] said:[6] "If all the parts of the terrestrial globe be magnetical, then a body, at a considerable depth below the surface of the earth, should lose somewhat of its gravitation or endeavor downwards by the attraction of the earth placed above it. This opinion some experiments made by some worthy persons of this honorable society seem to countenance"; but he pointed out the difficulty of conducting them in a conclusive manner and referred to the trial by clocks at certain depths and heights which, as Whewell[7] remarked, had been previously suggested by Bacon.

When Newton's temper was roused, he did not delay to imagine that Hooke might be a plagiarist;[8] but he was easily pacified.[9] Still, even then, he did not allow generosity to occupy too large a place in his just acknowledg-

[3] *Op. cit.*, p. xxix. In 1685, said Edleston, Newton "determines the attractions of masses and thus completes the demonstration of the law of universal gravitation."

[4] Rouse Ball, *op. cit.*, p. 157.

[5] See Section II of "Robert Hooke as a Precursor of Newton," *Monist*, July, 1913. Cf. Rigaud, *op. cit.*, pp. 56-58.

[6] Birch, *op. cit.*, Vol. II, p. 70.

[7] *History of the Inductive Sciences*, Vol. II, p. 173.

[8] Rouse Ball, *op. cit.*, p. 160.

[9] *Ibid.*, p. 165.

ments.[10] Newton's suspicions seem to have been very easily roused with regard to both enemies and friends. Worse than this is the way in which Newton, doubtless in one of his irritable fits, laid implicit or explicit claim to merits which he did not possess at the time under discussion. In a more generous moment he admitted[11] that Hooke was correct about the spiral; but, in the angry postscript, he said:[12]

"For his extending the duplicate proportion down to the center (which I do not) made him correct me, and tell me the rest of his theory as a new thing to me, and now stand upon it that I had all that from his letter, notwithstanding that he had told it to all the world before, and I had seen it in his printed books, all but the proportion."

Does not this imply that Newton was too cautious to consider the path below the surface of the earth? We know, from the letter he wrote to Hooke in 1679 and which is reproduced above, that he was not so cautious then. He had no inkling then of what he knew in 1686.

We must remember that this is not an isolated case. If it were, we would not suspect Newton of a slight meanness on no better evidence. But to throw discredit on Leibniz's infinitesimal calculus, he denied that he had ever used infinitesimal methods. On this De Morgan[13] remarked:

"We are of opinion that the *moral intellect* of Newton —not his moral *intention*, but his power of judging— underwent a gradual deterioration from the time when he settled in London. We see the faint traces of it in his manner of repudiation of the *infinitesimal* view of fluxions, in 1704. A man of sound judgment as to what is right

[10] *Ibid.*, pp. 165-167.

[11] *Ibid.*, pp. 165, 167. See above, §§ I-III, V.

[12] *Ibid.*, p. 160.

[13] Review of Brewster in *North British Review*, Vol. XXIII, 1855, pp. 324-325. This review is included in De Morgan's *Essays on the Life and Work of Newton*, just published by the Open Court Publishing Company.

does not abandon a view which he has held in common with a great rival, and this just at a time when the world is beginning to ask which came first in their common discovery, without a clear admission of the abandonment: he does not imply that *some* have held that view, and declare against the opinion of those *some,* without a distinct statement that he himself had been one of them: still less does he quietly and secretly alter what he had previously published, or allowed to be published, so as to turn the old view into the new one, and to leave the reader to understand that he had never changed his opinion. The Newton of the mythologists would have felt to his fingers' ends that such a proceeding had a tendency to give false impressions as to the case, and to throw suspicion on his own motives. This is a small matter, but it is a commencement of worse. We come to the *Commercium epistolicum,* the name given to the collection of letters accompanied by notes and a decision of the question, on the part of a Committee of the Royal Society. To this well-known part of the history Sir D. Brewster has a very important addition to make; and he makes it fairly, though we confess we wish he had given us what they call chapter and verse. 'It is due to historical truth to state that Newton supplied all the materials for the *Commercium epistolicum,* and that, though Keill was its editor, and the Committee of the Royal Society the authors of the Report, Newton was virtually responsible for its contents.' "

VII.

In the angry postscript to his letter written to Halley on June 20, 1686,[1] Newton seems to have laid his finger on the exact spot of Hooke's failure and his success:

"For it is plain, by his [Hooke's] words, that he knew now how to go about it. Now is not this very fine? Mathe-

[1] Rouse Ball, *op. cit.,* pp. 159-160.

maticians that find out, settle, and do all the business, must content themselves with being nothing but dry calculators and drudges; and another that does nothing but pretend and grasp at all things must carry away all the invention, as well of those that were to follow him as of those that went before. Much after the same manner were his letters writ to me, telling me that gravity, in descent from hence to the center of the earth, was reciprocally in a duplicate ratio of the altitude, that the figure described by projectiles in this region would be an ellipsis, and that all the motions of the heavens were thus to be accounted for; and this he did in such a way as if he had found out all, and knew it most certainly. And upon this information I must now acknowledge in print that I had all from him, and so did nothing myself but drudge in calculating, demonstrating, and writing, upon the inventions of this great man."

VIII.

We now leave Hooke and come to the incidents of 1684.

As Newton observed in the scholium to the fourth proposition of the first book of the *Principia,* Sir Christopher Wren, Hooke, and Halley had each observed that, in the case of the celestial bodies, "if the periodic times are in the sesquiplicate ratio of the radii, and therefore the velocities reciprocally in the subduplicate ratio of the radii, the centripetal forces will be in the duplicate ratio of the radii inversely: and the contrary." The facts were given in a letter from Halley to Newton dated June 29, 1686:[1]

"According to your desire in your former, I waited upon Sir Christopher Wren to inquire of him if he had the first notion of the reciprocal duplicate proportion from Mr. Hooke. His answer was that he himself very many years since had had his thoughts upon the making out of the planets' motions by a composition of a descent towards the

[1] Brewster, *op. cit.,* Vol. I, pp. 293, 296-298; Rouse Ball, *op. cit.,* pp. 25-36, 162-163.

sun and an impressed motion; but that at length he gave over, not finding the means of doing it. Since which time Mr. Hooke had frequently told him that he had done it, and attempted to make it out to him, but that he never satisfied him that his demonstrations were cogent. And this I know to be true, that in January, 1683/4 I, having from the consideration of the sesquialter proportion of Kepler, concluded that the centripetal force decreased in the proportion of the squares of the distances reciprocally, came one Wednesday to town, where I met with Sir Christopher Wren and Mr. Hooke, and, falling into discourse about it, Mr. Hooke affirmed that upon that principle all the laws of the celestial motions were to be demonstrated, and that he himself had done it. I declared the ill success of my attempts; and Sir Christopher, to encourage the inquiry, said that he would give Mr. Hooke or me two months' time to bring him a convincing demonstration thereof, and, besides the honor, he of us that did it should have from him a present of a book of forty shillings. Mr. Hooke then said that he had it, but he would conceal it for some time, that others, trying and failing, might know how to value it when he should make it public. However, I remember Sir Christopher was little satisfied that he could do it, and though Mr. Hooke then promised to show it him, I do not yet find that in that particular he has been as good as his word. The August following, when I did myself the honor to visit you, I learnt the good news that you had brought this demonstration to perfection, and you were pleased to promise me a copy thereof, which, the November following, I received with a great deal of satisfaction from Mr. Paget; and thereupon took another journey down to Cambridge on purpose to confer with you about it, since which time it has been entered upon the register books of the Society. As all this time past Mr. Hooke was acquainted with it, and, according to the philosoph-

ically ambitious temper he is of, he would, had he been master of a like demonstration, no longer have concealed it, the reason he told Sir Christopher and me now ceasing. But now he says that this is but one small part of an excellent system of nature which he has conceived, but has not yet completely made out, so that he thinks not fit to publish one part without the other. But I have plainly told him that unless he produce another differing demonstration and let the world judge of it, neither I nor any one else can believe it."

When in the August of 1684[2] Halley[3] visited Newton, he, without mentioning any of these speculations, "at once indicated the object of his visit by asking Newton what would be the curve described by the planets on the supposition that gravity diminished as the square of the distance. Newton immediately answered, 'an ellipse.' Struck with joy and amazement Halley asked him how he knew it. 'Why,' replied he, 'I have calculated it'; and, being asked for the calculation, he could not find it, but promised to send it to him. After Halley left Cambridge, Newton endeavored to reproduce the calculation, but did not succeed in obtaining the same result. Upon examining carefully his diagram and calculation, he found that in describing an ellipse coarsely with his own hand he had drawn the two axes of the curve instead of two conjugate diameters somewhat inclined to one another. When this mistake was corrected he obtained the result which he had announced to Halley."[4]

Newton sent the propositions, which were presumably on the conservation of areas under a central force, the law

[2] On the origin of the mistake sometimes made of taking 1683 for 1684, see Brewster, *op. cit.*, Vol. I, note on pp. 296-297; Rouse Ball, *op. cit.*, pp. 32-33.

[3] It must be remembered that an immense debt is owing to Edmund Halley both for the unwearied tact which he showed in persuading Newton to overcome his reluctance to publish, and for his generosity in paying for the publication when he could ill afford it (*ibid.*, pp. 67-68).

[4] Brewster, *op. cit.*, p. 297; Rouse Ball, *op. cit.*, p. 26.

of force in a focus under which an ellipse can be described, and the converse on the orbit described by a particle under a central force which varies inversely as the square of the distance,[5] to Halley in November, 1684; and Halley, on his return from his second visit to Newton, gave at the meeting of the Royal Society on Dec. 10, "an account that he had lately seen Mr. Newton at Cambridge, who had showed him a curious treatise, *De motu*; which, upon Mr. Halley's desire, was, he said, promised to be sent to the Society to be entered upon their register. Mr. Halley was desired to put Mr. Newton in mind of his promise for the securing his invention to himself till such time as he could be at leisure to publish it."[6]

Newton seems to have been stimulated by Halley's visit in August to continue his investigations. In the Michaelmas term of 1684 he read, as his professorial lectures, from a manuscript which is still extant and which bears the title *De motu corporum.* Whether this was the "curious tract" seen by Halley, or whether Halley saw the *Propositiones de motu,*[7] which was afterwards registered on the books of the Royal Society, is a question which has been much discussed,[8] but which is unimportant from the point of view of the development of the principles of mechanics. For the fact is that, in the first three lectures, the definitions and laws of motion are given exactly in the form in which they were stated in the *Principia.* Of these lectures it might be said with far more justification than has been said by Rouse Ball[9] of the tract *Propositiones de motu,* that they are "memorable as marking the point at which Newton had arrived about the end of 1684." I will discuss

[5] Rouse Ball, *op. cit.,* pp. 26-27. The propositions are I, XI, and XIII (Cor. I) of the first Book of the *Principia.*

[6] Rouse Ball, *op. cit.,* p. 30.

[7] See Rouse Ball, *op. cit.,* pp. 31, 33-51 (where it is printed).

[8] See *ibid.,* pp. 30-33; Brewster, *op. cit.,* pp. 299-300.

[9] *Op. cit.,* p. 33.

Newton's lectures in the next section; in the following section, the parts of the above tract which relate to the principle and the manuscript drafts of it in the Cambridge University Library will be described.

Here may be put in a small bit of evidence that Newton did not send the tract to Halley in November, but only an informal communication. Newton wrote to Halley on May 27, 1686: "In the papers in your hands there is not one proposition to which he [Hooke] can pretend."[10]

IX.

Newton had been appointed Lucasian Professor in the University of Cambridge in 1669, and first lectured on mechanics in the Michaelmas term of 1684.[1] The manuscript of these lectures is preserved at the University Library in Cambridge[2] in a volume containing the book-plate of King George the First, probably inserted by mistake, as the volume could not have been part of Bishop Moore's library.[3]

The reason of the appearance of King George the First's book-plate in the books from Bishop Moore's library in the University Library of Cambridge is as follows:[4] In 1715 the university sent up a loyal address to King George I on his accession, and the king, as a reward, bought the library of Dr. John Moore (1646-1714, Bishop of Norwich and afterwards of Ely), and presented it to the university. About the same time a troop of horse was quartered at Oxford in consequence of a Jacobite disturbance on the anniversary of the Pretender's birth. Hence

[10] *Ibid.*, p .155.

[1] On these and Netwon's other lectures, see Rouse Ball, *op. cit.*, pp. 27-30.

[2] Class-mark: "Dd. 9. 46." The Contents are described by Edleston (*op. cit.*, pp. xcv-xcvii) and Rouse Ball.

[3] Henry Bradshaw (*Collected Papers of Henry Bradshaw, late Universty Librarian*, Cambridge, 1889, p. 201) wrote that there are no inventories of Bishop Moore's library preserved, not even of the manuscripts.

[4] *In Praise of Cambridge: an Anthology in Prose and Verse*, selected and edited by Sydney Waterlow, London, 1912, pp. 14, 210.

the following epigram by an Oxford wit, probably Dr. Trapp or Thomas Warton:

> "The king observing with judicious eyes,
> The state of both his universities,
> To one hē sends a regiment; for why?
> That learned body wanted loyalty.
> To th'other books he gave, as well discerning
> How much that loyal body wanted learning."

Dr. William Browne (1692-1774), according to Mrs. Piozzi, heard Dr. Samuel Johnson quote this, and thereupon composed impromptu:

> "The king to Oxford sent his troop of horse,
> For Tories own no argument but force.
> With equal care, to Cambridge books he sent:
> For Whigs allow no force but argument."

"Mr. Johnson," says Mrs. Piozzi, "did Browne the justice to say it was one of the happiest extemporaneous productions he ever met with; though he once comically confessed that he hated to repeat the wit of a Whig in support of Whiggism."

X.

These lectures contain the definitions, laws of motion, the first section, and the first 35 propositions of the *Principia*. It must be remembered that the third law of motion of the *Principia* is not used until the eleventh section of that work, which deals with the motion of two or more bodies under their mutual attractions, and begins with Proposition LVII. In the tract sent to the Royal Society, there is, as we shall see in the next section, no mention at all of the third law; but there is in one of the unpublished drafts of this manuscript. From another manuscript in the Portsmouth Collection, which has not, I think, been particularly noticed before, there is a statement of the third

law which seems to carry its recognition back to an earlier period of Newton's life.

Brewster[1] merely said of the Royal Society's tract that "it occupies twenty-four octavo pages, and consists of four theorems and seven problems, four of the theorems and four of the problems containing the more important truths which are demonstrated in the second and third sections of the first book of the *Principia*"; and neither by Brewster, Rosenberger,[2] nor by any historian of mechanics or mathematics, so far as I know, is pointed out the fact that the third law of motion appears not at all in the Royal Society's manuscript; or that it appears in a different form to that of the *Principia*, in a manuscript in the Portsmouth Collection written shortly after this.

XI.

In the Royal Society's tract[1] there are three definitions, (centripetal force, force of a body [of inertia], and resistance) and[2] four "Hypotheses":

"1. In the next nine propositions the resistance is to be zero, in the following ones it is to be conjointly as the density of the medium and the velocity;

"2. Every body, by the force implanted in it, moves to infinity uniformly in a straight line unless something outside it prevents it;

"3. A body in a given time, under the joint action of forces, is carried to the same place as by separate forces in successive equal times;

[1] *Op. cit.*, Vol. I, p. 299. Cf. Rosenberger, *op. cit.*, p. 159.

[2] When speaking of the third law of motion on pp 176-177 (*op cit.*).

[1] Reproduced on pp. 35-51 of Rouse Ball's book.

[2] "1. Resistentiam in proximis novem propositionibus nullam esse, in sequentibus esse ut medii densitas et celeritas conjunctim.

"2. Corpus omne sola vi insita uniformiter secundum lineam rectam in infinitum progredi, nisi aliquid extrinsecus impediat.

"3. Corpus in dato tempore, viribus conjunctis, eo ferri quo viribus divisis in temporibus aequalibus successive.

"4. Spatium quod corpus, urgente quacunque vi centripeta, ipso motus initio describit, esse in duplicata ratione temporis" (*ibid.*, p. 36).

"4. The space which a body, being urged by some centripetal force, describes from the same beginning of motion, is in the duplicate ratio of the time."

The propositions correspond[3] to those in the *Principia,* Book I, Propositions 1, 4, 6, 7, 10, 11, 15, 17, 32, and Book II, Propositions 2, 3, and 4, and deal with the motion of particles about fixed centers of force in non-resisting and resisting media. There is nothing corresponding to the third law of motion here.

But there is in the Portsmouth Collection[4] a manuscript draft of the tract, which appears to me to be of later date than the other two. It is somewhat fuller and bears the heading, which perhaps is of a later date than the manuscript and possibly not in Newton's handwriting: *De motu sphaericorum corporum in fluidibus,*[5] and contains a different form of the third law of motion. In this manuscript there are five laws,[6] the term "hypothesis" here and in the subsequent references in the manuscript being crossed out and *lex* written. These are as follows:[7]

"Law 1. A body always proceeds, by the force implanted in it alone, uniformly in a straight line if nothing prevents it;

"Law 2. Change of state of motion or rest is proportional to the impressed force and takes place in the straight line in which that force is impressed;

[3] Cf. *ibid.,* pp. 33-34.

[4] On two other drafts of the tract, see *ibid.,* p. 51.

[5] Reproduced on pp. 51-56 (*ibid.*).

[6] *Ibid.,* p. 52. Cf. the application of the third and fourth laws translated below (from pp. 54-55).

[7] "Lex 1. Sola vi insita corpus uniformiter in linea recta semper pergere si nil impediat.

"Lex 2. Mutationem status movendi vel quiescendi proportionalem esse vi impressae et fieri secundum lineam rectam qua vis illa imprimitur.

"Lex 3. Corporum dato spatio inclusorum eosdem esse motus inter se sive spatium illud quiescat sive moveat id perpetuo et uniformiter in directum absque motu circulari.

"Lex 4. Mutuis corporum actionibus commune centrum gravitatis non mutare statum suum motus vel quietis. Constat ex Lege 3.

"Lex 5. Resistentiam medii esse ut medii illius densitas, et corporis moti sphaerica superficies et velocitas conjunctim" (*ibid.* p. 52).

"Law 3. The motions with respect to one another of bodies included in a given space are the same whether the space is at rest or moves perpetually and uniformly in a straight line without a circular motion;

"Law 4. By the mutual action of bodies the common center of gravity does not change its state of motion or rest. Proved from Law 3.

"Law 5. The resistance of a medium is proportional conjointly to the density of the medium, the spherical surface of the body moved, and the velocity."

At the end of Law 4, after "Constat ex," apparently "ex lege 3" was written, crossed out, and then rewritten. Law 3 is the same as Cor. V and Law 4 as Cor. IV to the Laws of Motion of the *Principia*.

In this manuscript there is an addition[8] of which this is a translation:

"The whole remaining space of the planetary heavens either is at rest (as is commonly believed) or moves uniformly in a straight line, and therefore the common center of gravity of the planets (by Law 4) either is at rest or moves with it. In both cases the motions of the planets relative to one another take place (by Law 3) in the same manner, and their common center of gravity is at rest with respect to the whole of space and ought therefore to be considered as the motionless center of the whole planetary system. Hence, in truth, the Copernican system is proved *a priori*. For if the common center of gravity be computed for any position of the planets, it will either fall in the body of the sun or will always be near it. Owing to this deviation of the sun from the center of gravity, it comes about that the centripetal force does not always tend towards the motionless center, and consequently that the planets neither move exactly in ellipses nor revolve twice in the same orbit. There are as many orbits of each planet as there are rev-

[8] *Ibid.*, pp. 54-55.

olutions, as is the case with the moon's motion, and each several orbit depends on the conjoint motions of all the planets, to say nothing of the mutual actions which they all exert on one another. But to consider at once so many causes of motions and to define the motions themselves by exact laws admitting of convenient calculation surpasses, if I mistake not, the power of any human ingenuity. Neglecting these details, the orbit will be simple, and, as the mean of all the errors, will be the ellipse of which I have treated already. If anyone should try to determine this ellipse from three observations by a trigonometrical computation (as is usual), he will have approached the matter without sufficient caution. The observations will participate in the minute irregularities of the motions which are here neglected, and will therefore cause the ellipse to deviate a little from its just magnitude and position (which ought to be the mean of all the errors), and will give as many ellipses discrepant with one another as sets of three observations are admitted. Therefore as many observations as possible ought to be conjoined and compared by one operation, that these operations may mutually correct one another and exhibit the ellipse as a mean in position and magnitude."

XII.

We will now turn our attention to the other manuscripts of Newton's preserved in the University Library of Cambridge.

Among the early papers by Newton (holograph) in Section I, Division I, of the Portsmouth Collection[1] are:

3. A tract in English written in 1666 and entitled: *To Resolve Problems by Motion.* Also short tracts entitled:

De solutione problematum per motum;

[1] *Portsmouth Catalogue,* p. 1. The numbers 3-5 refer to items in this *Catalogue.*

De gravitate conicarum;
Problems of Curves.

4. *Calculation of the Area of the Hyperbola.*[2]

5. *On the Laws of Motion*;
On the Laws of Reflection;
On Motion in a Cycloid.

The tract *To Resolve Problems by Motion* is dated—apparently by Newton at a later date—October, 1666, and is concerned, not with mechanics, but with the use of the conception of motion in geometry.

In the margins of the tract *On the Laws of Motion* in No. 5 are:

1. Of place, motion, velocity and force;

2. With what velocity a body moves several ways at once;

3. How two progressive motions are joined into one [Parallelogram of motions];

4. Of centers and axes of motion and the motion of those centers.

5. Of circular motion and velocity about those axes;

6. With what velocity a body circulates about several axes at once;

7. How two circular motions are joined into one;

8. In what cases a circulating body perseveres in the same state and in what it doth not.

How bodies are reflected.

9. Some names and letters defined;

10. The Rule for Reflection;

11. The Conclusion;

12. In what method the precedent rules must be used.

The tract concludes with "Some observations about motion." In the first section occur the words:

"Which motion is done with more or less velocity ac-

[2] This was an early calculation of Newton's performed at Boothby in Lincolnshire.

cordingly as it is done through more or less space in equal times or through equal spaces in more or less time. But the motion itself and the force to persevere to the motion is more or less accordingly as the factors of the body's bulk into its velocity is more or less. And that force is equivalent to that motion it is able to beget or destroy."

Again, the fourth section is:

"In every body there is a certain point, called its center of motion, about which if the body be any way circulated, the endeavors of its points every way from the center are exactly counterpoised by opposite endeavors. And the progressive motion of the body is the same with the motion of this its center which always moves in a straight line and uniformly with other bodies. And so doth the common center of two bodies, which is found by dividing the distance between their proper centers in reciprocal proportion to their bulk. And so the common center of three or more bodies, etc. And all the lines passing through these centers of motion are axes of motion."

The tract *On the Laws of Reflection* begins in the same way as above.

In Section VIII, No. 5,[3] there is the beginning of "The Elements of Mechanicks." This tract begins as follows:

"1. All bodies continue perpetually in their state of resting or moving in right lines unless disturbed by any force, and, when disturbed, the changes of these states are proportional to the force disturbing them. And if this change and the antecedent motion of the body be represented in situation and quantity by the sides of a parallelogram, the motion arising from them both shall be represented by the diagonal of that parallelogram.

"2. The actions of two bodies upon one another are always equal and directly contrary, and the resiliation of clashing bodies is as their springiness, and the force of a

[3] *Portsmouth Catalogue*, p. 49.

globular body upon a resting plane is as the part of its velocity which is perpendicular to the plane.

"3. The forces with which the parts of one and the same engine move at one and the same time are reciprocally proportional to their velocities.

"4. In a system of bodies moving variously amongst one another and acting upon one another by any other bodies, the common center of gravity of them all either rests perpetually or moves uniformly in a right line, and, whether it rests or moves, the motions of the bodies amongst one another are the same.

"5. Falling bodies describe spaces proportional to the squares of the times, and so do projectiles by their motions of ascent and descent, but their horizontal motions are uniform and with both motions together they describe parabolas, and water spouts up in a parabolic figure after the manner of projectiles."

These extracts show that Newton's conception of mass as quantity of matter dated from his early days. Indeed, it is quite likely that this conception was possessed by others[4] than Newton before Newton's time, but in an unformulated fashion. If we examine the basis of this conception, we see that the conception rests upon the implicit postulate of the sameness, in essentials, of all matter.

[4] I am thinking more particularly of Wallis. Besides the reference to him in "Robert Hooke as a Precursor of Newton" (*Monist,* July, 1913), cf. the following passage from Edleston, *op. cit.,* pp. 300-301, from a letter which Wallis wrote to Newton on April 10, 1695: "I understood you are now about adjusting the moon's motions; and, amongst the rest, take notice of that of the *common center of gravity* of the earth and moon as a conjunct body (a notion which, I think, was first started by me, in my Discourse of the Flux and Reflux of the Sea). And it must needs be of a like consideration in that of Jupiter with his Satellites, and of Saturn with his. (And I wonder we have not yet heard of any about the moon.) But, Saturn and Jupiter being so far off, the effects thereof are less observable by us than that of the moon. My advice upon the whole, is, that you would not be too slow in publishing what you do." In Siegmund Günther's article "Histoire des origines de la loi Newtonienne de la gravitation" (Bibliothèque du Congrès internationale de Philosophie, III: *Logique et histoire des sciences,* Paris, 1901, pp. 49-75), which is very rich in references, there is on p. 63 a statement that Fermat was acquainted with the principle of the equivalence of action and reaction, and also with the fact that a material point is more feebly attracted inside a sphere than outside.

There seems to be a natural bent of the human mind to start from, or arrive at, this postulate. Descartes, who had the philosopher's quality of being conscious of some at least of the postulates on which his system was founded, started from the hypothesis of a homogeneous *plenum,* and his example is being constantly followed by those physicists of even our own day who are unacquainted with the history of thought. Hooke, as we have seen, kept the *plenum* but believed in qualitative differences in matter. Newton seems to have believed in the ultimate sameness of constitution of matter, and hence must have early freed himself from Cartesian opinions, if he ever held them. He seems to have been a follower of Robert Boyle's "corpuscular philosophy."[5]

XIII.

In order to compare these earlier statements of Newton's fundamental principles of dynamics with those in his *Principia* of 1687, it will be as well to consult the translation by Andrew Motte[1] of Newton's final statement of the "Axioms or Laws of Motion."

I will only make two observations on this later form of Newton's Laws of Motion.

In that manuscript of Newton's which is the second dealt with in §XI above, what is there called "Law 4" is stated to be proved by "Law 3." Now, "Law 3" is the same as the fifth corollary to the Laws of Motion of the

[5] On Descartes's physical theory and for a criticism of it, see Kurd Lasswitz, *Geschichte der Atomistik vom Mittelalter bis Newton,* Vol. II, Hamburg and Leipsic, 1890, pp. 55-126. By "density" (*soliditas*) Descartes (*Principia,* 1644, III, 121) understood the quantity (*quantitas*) of his "third element"—which forms the enveloping crust of the planets—as compared with its total magnitude (*moles*) and surface (see Lasswitz, *op. cit.,* pp. 70-72). On Boyle's corpuscular theory, see *ibid.,* pp. 261-293. On Huygens's work, see *ibid.,* pp. 341-397. Finally, on Newton's physics, see *ibid.,* pp. 555-580.

[1] *The Mathematical Principles of Natural Philosophy*; 2 vols. London, 1729. The laws of motion are on pp. 19-40 of the first volume. American editions, New York, 1848, 1850.

Principia: "The motions of bodies included in a given space are the same among themselves whether that space is at rest or moves uniformly forwards in a right line without any circular motion." Newton deduced this corollary from the second law of the *Principia*. "Law 4" of the manuscript is the same as the fourth corollary to the Laws of Motion of the *Principia*: "The common center of gravity of two or more bodies does not alter its state of motion or rest by the actions of the bodies among themselves; and therefore the common center of gravity of all bodies acting upon each other (excluding outward actions and impediments) is either at rest or moves uniformly in a right line." This corollary is a simple deduction from the third Law of the *Principia*, and cannot, as Newton appeared to think in the manuscript referred to, be deduced from the "Law 3" of that manuscript.

XIV.

In the second place, Newton had a most interesting passage in the scholium to the Laws of Motion. After showing that "the third law, so far as it regards percussions and reflexions, is proved by a theory exactly agreeing with experience," he proceeded as follows:

"In attractions I briefly demonstrate the thing after this manner. Suppose an obstacle is interposed to hinder the congress of any two bodies A, B, mutually attracting each other, then if either body, as A, is more attracted towards the other body B, than that other body B is towards the first body A, the obstacle will be more strongly urged by the pressure of the body A than by the pressure of the body B, and therefore will not remain in equilibrium: but the stronger pressure will prevail, and will make the system of the two bodies, together with the obstacle, move directly towards the side on which B lies; and in free space to go forward to infinity with a motion perpetually acceler-

ated—which is absurd, and contrary to the first law. For, by the first law, the system ought to persevere in its state of rest or of uniform motion forwards in a right line; and therefore the bodies must equally press the obstacle, and be equally attracted one by the other. I made the experiment on the loadstone and iron. If these are placed apart in proper vessels and made to float by one another in standing water, neither of them will propel the other, but by being equally attracted they will sustain each other's pressure and rest at last in equilibrium.

"So[1] the gravitation between the earth and its parts, is mutual. Let the Earth FI be cut by any plane EG into two parts EGF and EGI: their weights one towards the other will be mutually equal. For if, by another plane HK, parallel to the former EG, the greater part EGI is cut into two parts EGKH and HKI, whereof HKI is equal to the part EFG first cut off: it is evident that the middle part EGKH will have no propensity by its proper weight towards either side, but will hang as it were at rest and in equilibrium between both. But the one extreme part HKI will with its whole weight bear upon and press the middle part towards the other extreme part EGF; and therefore the force with which EGI, the sum of the parts HKI and EGKH, tends towards the third part EGF, is equal to the weight of the part HKI, that is, to the weight of the third part EGF. And therefore the weights of the two parts EGI and EGF, one towards the other, are equal, as I was to prove. And indeed if those weights were not equal, since the whole earth floats in the non-resisting ether, it would give way to the greater weight, and, retiring from it, would be carried off to infinity."

This passage is particularly interesting because it contains the same form of argument—the excluded *perpetuum*

[1] This paragraph first appeared in the second edition (1713) of the *Principia.*

mobile—that, as Mach[2] has shown, Stevinus, Galileo, Huygens, Torricelli, John Bernoulli, Lagrange, Sadi Carnot, Julius Robert Mayer, Franz Neumann, and Helmholtz used as the foundation both of mechanics and of other parts of physics. That this remark of Newton's was not referred to by Mach makes it all the more necessary to refer to it here.

Further to illustrate this point, we will consider a part of the correspondence of Cotes and Newton, when Cotes was preparing the second edition of the *Principia*.[3]

Cotes, when giving Newton, in a letter of March 18, 1713, an account of the Preface he meant to write, commented as follows on Prop. V of Book III:[4]

"But, in the first corollary of this fifth proposition, I meet with a difficulty; it lies in these words: *Et cum attractio omnis mutua sit.* I am persuaded that they are then true when the attraction may properly be so called, otherwise they may be false. You will understand my meaning by an example. Suppose two globes A and B placed at a distance from each other upon a table, and that, whilst the globe A remains at rest, the globe B is moved towards it by an invisible hand; a bystander, who observes this motion but not the cause of it, will say that the globe B does certainly tend to the center of globe A, and thereupon he may call the force of the invisible hand the centripetal

[2] See the English translation of his publication of 1872. This translation is entitled: *History and Root of the Principle of the Conservation of Energy* (Chicago: Open Court Publishing Co., 1911).

[3] In all of these letters, since the interest is scientific rather than literary, I have modernized dates and spelling, and translated Latin sentences, for the convenience in reading and printing.

[4] Edleston, *op. cit.*, pp. 152-153, The proposition referred to is: "That the circumjovial planets gravitate towards Jupiter, the circumsaturnal towards Saturn, the circumsolar towards the sun; and, by the forces of their gravity, are drawn off from rectilinear motions and retained in curvilinear orbits." The first corollary is: "There is therefore a power of gravity tending to all the planets. For doubtless Venus, Mercury, and the rest are bodies of the same sort as Jupiter and Saturn. And, since all attraction (by the third law) is mutual, Jupiter will therefore gravitate towards all his own satellites, Saturn towards his, the earth towards the moon, and the sun towards all the primary planets."

force of B and the attraction of A, since the effect appears the same as if it did truly proceed from a proper and real attraction of A. But then I think he cannot, by virtue of the axiom *Attractio omnis mutua est*, conclude, contrary to his sense and observation, that the globe A does also move towards the globe B and will meet it at the common center of gravity of both bodies. This is what stops me in the train of reasoning by which I would make out, as I said, in a popular way, your seventh proposition of the third book. I shall be glad to have your resolution of the difficulty, for such I take it to be. If it appears so to you also, I think it should be obviated in the last sheet of your book, which is not yet printed off, or by an Addendum to be printed with the Errata table. For, until this objection be cleared, I would not undertake to answer any one who should assert that you do *hypothesim fingere*. I think you seem tacitly to make this supposition that the attractive force resides in the central body."

On March 28 Newton replied:[5]

"The difficulty you mention, which lies in these words: '*Et cum attractio omnis mutua sit,*' is removed by considering that, as in geometry the word 'hypothesis' is not taken in so large a sense as to include the axioms and postulates, so in experimental philosophy it is not to be taken in so large a sense as to include the first principles or axioms which I call the Laws of Motion. These principles are deduced from phenomena and made general by induction, which is the highest evidence that a proposition can have in this philosophy. And the word 'hypothesis' is here used by me to signify only such a proposition as is not a phenomenon nor deduced from any phenomena, but assumed or supposed without any experimental proof. Now the mutual and mutually equal attraction of bodies is a

[5] *Ibid.*, pp. 154-155.

branch of the third Law of Motion, and how this branch is deduced from phenomena you may see in the end of the corollaries of the Laws of Motion. If a body attracts another body contiguous to it and is not mutually attracted by the other, the attracted body will drive the other before it and both will go away together with an accelerated motion to infinity, as it were by a self-moving principle, contrary to the first Law of Motion, whereas there is no such phenomenon in all nature.

"At the end of the last paragraph but two now ready to be printed off,[6] I desire you to add after the words: 'a God without dominion, providence, and final causes, is nothing else but fate and nature' these words: 'And thus much concerning God; to discourse of whom from the appearances of things does certainly belong to natural philosophy.'

"And, for preventing exceptions against the use of the word 'hypothesis,' I desire you to conclude the next paragraph in this manner: 'For whatever is not deduced from the phenomena, is to be called a hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy. In this philosophy particular propositions are inferred from the phenomena, and afterwards rendered general by induction. Thus it was that the impenetrability, the mobility, and the impulsive force of bodies, and the laws of motion and of gravitation, were discovered. And to us it is enough that gravity does really exist, and act according to the laws which we have explained, and abundantly serves to account for all the motions of the celestial bodies and of our sea.' "

[6] This is in the "General Scholium" at the end of the *Principia*.

XV.

In his *Principia,* Newton[1] defined "quantity of matter" as the product of volume and density. He gave no further explanation of the conception of density; but from what he said later on it appears that he supposed the smallest particles of matter to be equally dense and simply put the density proportional to the number of these particles in a given space.[2]

When giving an account of the eleventh section of the first book of the *Principia,* Rosenberger[3] remarked: "Since all the corpuscles of which bodies consist contain equal quantities of matter, and since all attractions are wholly due to matter, all corpuscles exert equal forces outwards. The total force of a body is thus the sum of the forces of the corpuscles, the action of a body outwards must, without exception, be proportional to the number of its corpuscles, and the weight of a body gives a sure measure of its mass. This idea of a body as a system of corpuscles whose unity only becomes evident with bodies that are very distant from one another, forms the transition from the mechanics of points to the mechanics of systems of points or physical bodies. The indication of this transition is, as is always the case with Newton, very slight. 'It is reasonable to suppose,' said he in the scholium at the end of the eleventh section, 'that forces which are directed to bodies should depend upon the nature and quantity of those bodies, as we see they do in magnetical experiments. And when such cases occur, we are to calculate the attractions of the bodies by assigning to each of their particles its proper force, and then collecting the sum of them all.' "

[1] Book I, Definition I. "It is this quantity that I mean hereafter everywhere under the name of body or mass."

[2] Rosenberger, *op. cit.,* p. 173.

[3] *Ibid.,* p. 192. It must be remembered that the conception of mass and the third law of motion were first used in this eleventh section, which deals with the motions of bodies tending to each other with centripetal forces.

The sixth proposition of the third book is: "That all bodies gravitate towards every planet; and that the weights of bodies towards any planet at equal distances from the center of the planet are proportional to the quantities of matter which they severally contain." To this proposition, the second corollary was: "Universally, all bodies about the earth gravitate towards the earth, and the weights of all bodies at equal distances from the earth's center are as the quantities of matter which they severally contain. This is the quality of all bodies within the reach of our experiments, and therefore, by the third Rule for Philosophizing, is to be affirmed of all bodies whatsoever." To this, Rosenberger[4] added the remark: "For the effect of the whole body is equal to the sum of the effects of its single particles, and therefore, if the particles are all equal, is proportional to the number of the particles or the mass." However, it must be emphasized that Newton nowhere stated explicitly that the mass was proportional to the number of particles. That he thought so, there can, I think, be little doubt.

In the third rule of the *regulae philosophandi* at the beginning of his third book,—the rule, namely, that: "The qualities of bodies, which admit neither intension nor remission of degrees, and which are found to belong to all bodies within the reach of our experiments, are to be esteemed the universal qualities of all bodies whatsoever,"—Newton included among the qualities which can neither be increased nor diminished: extension, hardness, impenetrability, mobility, and inertia, and remarked: "And, because the hardness of the whole arises from the hardness of the parts, we therefore justly infer the hardness of the undivided particles not only of the bodies we feel but of all others.... We conclude the least particles of all bodies to be also all extended, hard, impenetrable, mobile, and en-

[4] *Op. cit.*, p. 214.

dowed with their proper forces of inertia. And this is the foundation of all philosophy. Moreover, that the divided but contiguous particles of bodies may be separated from one another, is matter of observation; and, in the particles that remain undivided, our minds are able to distinguish yet lesser parts, as is mathematically demonstrated. But whether the parts so distinguished, and not yet divided, may, by the powers of nature, be actually divided and separated from one another, we cannot certainly determine. Yet, had we the proof of but one experiment, that any undivided particle, in breaking a hard and solid body, suffered a division, we might, by virtue of this rule, conclude that the undivided as well as the divided particles may be divided and actually separated to infinity."

The second corollary to the sixth proposition went on:

"If the ether, or any other body, were either altogether devoid of gravity or were to gravitate less in proportion to its quantity of matter, then, because, according[5] to Aristotle, Descartes, and others, there is no difference between that and other bodies but in the mere form of matter, it might, by a successive change from form to form, be changed at last into a body of the same condition as those which gravitate most in proportion to their quantity of matter; and, on the other hand, the heaviest bodies, acquiring the first form of that body, might by degrees quite lose their gravity. And therefore the weights would depend upon the forms of bodies, and might be changed with those forms, contrary to what was proved in the preceding [the first] corollary."

The third corollary will occupy us later, in connection with some of the correspondence between Cotes and Newton. The fourth corollary was: "If all the solid particles of all bodies are of the same density and cannot be rarefied

[5] This and the five following words were first added in the second edition of the *Principia.*

without pores, a void space or vaccum must be granted. By bodies of the same density I mean those whose forces of inertia are in the proportion of their bulks." Now, Rosenberger[6] went rather beyond the facts and remarked: "Newton therefore, in a fourth corollary stated that the solid particles of all bodies are equally dense and that bodies of different density only differ by the magnitude and quantity of their pores."

In the April of 1686, Halley[7] announced the appearance of Newton's *Principia* in an essay which appears to have been inspired by Newton and which, as Rosenberger[8] has remarked, "contains in short indications almost all the foundations of Newton's natural philosophy. Only one principle is still lacking, and that is the idea of molecular gravitation or the composition of the gravitation of bodies out of that of their smallest particles. Everywhere, Halley only speaks of the tendency of the single particles of the body towards a point inside the body—the center of gravitation—, but does not remark that this center is obtained by summation of the effects of all the particles, and that the center of force is only a mathematical fiction. Indeed, he even indicates that the Creator might have ordained this center to be anywhere in bodies, and this—so at least it appears to me—directly contradicts the idea of a molecular gravitation. However, it cannot be decided with certainty whether or no Newton himself, at the time at which Halley drew up his memoir, had formed the idea of molecular attraction and had merely not progressed with it so far that he could communicate it to others; or whether or no Newton had not then grasped the thoughts about molecular forces, and only arrived at this important notion after his communications to Halley. From the *Principia* of 1687, we can conclude that the idea of molecular gravitation together

[6] *Op. cit.*, p. 215.
[7] Cf. *ibid.*, p. 260, and Rouse Ball, *op. cit.*, p. 62.
[8] *Op. cit.*, p. 263

with the conceptions of mass, developed with Newton at a fairly late stage—when, in fact, he was busied with the elaboration of the attraction of extended bodies. Still, it is not very likely that this development was still uncompleted at the time of Halley's announcement."

On this, we may again remark that it is now definitely established that Newton did not discover the theorems on the attraction of spherical bodies until 1685, and apparently did not mention them to Halley until the letter of June 20, 1686, an extract from which is given above. On the other hand, the Newtonian conception of mass, as we have seen from Newton's early and previously unpublished manuscripts, was of much earlier growth. It is probable that Newton made then what seems now to be the obvious step —the way for which was amply prepared by Descartes— of considering the ultimate particles of all bodies to be of the same density, and all the differences of bodies to arise solely from the number of these particles in a given space; and it is not unlikely, as we saw above, that Newton early reached the idea of universal gravitation—of attractions between particles, which vary inversely as the squares of the distances and directly as the masses. If this be so, we must admit that Newton's physical views were, in essentials, completed at an early date, and that the subsequent development of his "philosophy" was preeminently of a mathematical character. Of course, the mathematical work enabled precision and a most attractive appearance of completeness to be given to these physical views. In particular, the purely mathematical determination of the attractions of spherical bodies seems to have played a very important part in this respect, as in others. I cannot, then, agree with Rosenberger as to the comparatively late growth of Newton's chief physical ideas. On another occasion I will trace the fortunes of Newton's views on the ether and on gravitation as an essential property of matter, and will

examine Rosenberger's opinions on these subjects. However, it seems to me that Rosenberger has correctly grasped the gist of the Newtonian conception of mass, though his methods of quotation are by no means always unprejudiced. It is important to try to get at Newton's exact meaning, for his notions about mass, ether, and whether or no gravitation is an essential property of matter, were never expressed without caution, which is doubtless commendable, and ambiguity, which is certainly not.

XVI.

Mechanical ideas were applied to celestial events, and not only to terrestrial events before Newton's time. Newton, of course, was the first to carry out this application in a thoroughly scientific manner, but good work in this direction had been done by Descartes, Wallis, Wren, Hooke, Halley, and others. With this extension of ideas came the idea of one and the same body—involving therefore something constant—having different weights according to its positions in space.[1] Mach referred with approval to some of Rosenberger's[2] remarks on the Newtonian conception of mass. "It should be observed," said Mach,[3] "that the notion of mass as quantity of matter was psychologically a very natural conception for Newton, with his peculiar development. Critical inquiries as to the origin of the concept of matter could not possibly be expected of a scientist in Newton's day. The concept developed quite instinctively; it is discovered as a datum perfectly complete, and is adopted with absolute ingenuousness. The same is the case with the concept of force. But force appears conjoined with matter. And, inasmuch as Newton invested all material particles with precisely identical gravitational forces, inasmuch as he regarded the forces exerted by the

[1] Cf. Mach, Mechanics, p. 192. Cf. also pp. 194-195, 216-222, 536-537, 558-560 for a criticism of the Newtonian conception of mass.

[2] *Op. cit.*, p. 192. [3] *Op. cit.*, p. 536.

heavenly bodies on one another as the sum of the forces of the individual particles composing them, naturally these forces appear to be inseparably conjoined with the quantity of matter."

On the enumeration of the ultimate particles to get the measure of mass, Mach[4] remarked: "We do not find the expression 'quantity of matter' adapted to explain and elucidate the concept of mass, since that expression itself is not possessed of the requisite clearness. And this is so, though we go back, as many authors have done, to an enumeration of the hypothetical atoms. We only complicate, in so doing, indefensible conceptions. If we place together a number of equal, chemically homogeneous bodies, we can, it may be granted, connect some clear idea with 'quantity of matter,' and we perceive also that the resistance the bodies offer to motion increases with this quantity. But the moment we suppose chemical heterogeneity, the assumption that there is still something that is measurable by the same standard, which something we call quantity of matter, may be suggested by mechanical experiences, but is an assumption nevertheless that needs to be justified." It must be remarked that Newton himself appears to be one of those authors referred to by Mach;[5] and that the Newtonian conception, involving as it does the hypothesis of the sameness in constitution of the ultimate particles of bodies which are chemically heterogeneous, is perfectly clear: the only defect is that it rests on mere hypothesis, which can be neither proved nor disproved. Indeed, it seems that Newton's boast—or expression of caution—*hypotheses non fingo* only applied to such hypotheses as he consciously and explicitly made.

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[4] *Ibid.*, pp. 216-217.

[5] Heinrich Hertz, in his well-known work of 1894 on the principles of mechanics, also defined mass as the number of particles.

THE DEVELOPMENT OF MAHAYANA BUDDHISM.

EUROPEAN Buddhist scholars are wont to divide Buddhism into two schools, Northern and Southern. By Southern Buddhism they understand that form which prevails mostly in Ceylon, Burma, and Siam; while Tibetan Lamaism, and Chinese, Korean, and Japanese Buddhism they consider as belonging to the Northern school. This geographical division, however, does not seem to be quite correct and justifiable; for we know that the Buddhism of Tibet is as different from the Buddhism of Japan as the latter is from that of Ceylon or Burma, not only in some of its teachings but principally in its practical aspects. Take, for instance, the Chinese or Japanese Zen sect or the sect of the Pure Land, and compare it to Tibetan Buddhism as it is known to us to-day, and we shall find that the difference between the two is wider perhaps than that between the so-called Southern Buddhism and the Japanese Buddhist sect called Risshu or Vinaya sect.

A better way of classifying the different schools of Buddhism is to divide them into the Buddhism of Arhats and the Buddhism of Bodhisatvas; understanding by the former that Buddhism whose ideal attainment is Arhatship, and by the Buddhism of Bodhisatvas that system of Buddhist teachings which makes the conception of Bodhisatvahood its most salient feature. Or we may retain the old way of classifying the followers of the Buddha into two groups: the Mahâsanghîka and the Sthâvira. Or,

further, we can even invent a new method of division, and call one the progressives and the other the conservatives.

Taking all in all, however, it seems to me that the division of Mahâyâna and Hînayâna Buddhism is preferable to all the rest from the point of view of our knowledge of Buddhism. Of course, this way of dividing Buddhism has its historical odium, which is most desirable for modern scholars of Buddhism to avoid. Except for this latter objection, the term "Mahâyâna" is comprehensive and definite enough to include all those schools of Buddhism in which the ideal of Bodhisatvahood is upheld in preference to the attainment of Arhatship, and whose geographical distribution covers not only the northern parts of India but east of them as well. Let us here, therefore, use the term "Mahâyâna" more for the sake of convenience than anything else, until we shall have studied Buddhism in all its diverse aspects, historic, dogmatic, and ritualistic, when we shall be able to understand Buddhism far better than we do now.

The object of my present article is to expound briefly what in my view constitutes the essential characteristics of Mahâyâna Buddhism in contradistinction to Hînayâna Buddhism.

The character of Mahâyâna Buddhism can be expressed in the one word, speculative. Buddhism generally teaches three forms of discipline: moral (*çîla*), contemplative (*dhyâna*), and intellectual (*prajñâ*); and of these the last seems to have been particularly emphasized by the Mahâyâna Buddhists, while the moral discipline has become the chief feature of Southern Buddhism, so called,—in fact, to such an extent that most Western scholars of Buddhism, whose principal source of information is the Pâli *Tipitaka,* are apt to take Buddhism for no more nor less than a system of ethical culture, which, therefore, cannot be called a religion in the same sense as is Christianity. While the

Buddha apparently taught a well-balanced practice of *çîla, dhyâna,* and *prajñâ,* his followers became one-sided as has been also generally the case with other religious systems, and emphasized one aspect at the expense of the others. The Mahâyâna in one sense can be said to have gone too far in its speculative flight, almost to the point of forgetting its ethical side, while the Hînayâna adherents have been so extremely conservative as to refuse to adapt themselves to an ever-changing environment. However this may be, a practical reformer of Buddhism to-day would do well if he endeavored to restore the three forms of discipline each in its proper bearings and thereby to manifest more perfectly the original spirit of the founder of Buddhism.

This one-sided tendency and the development of the two schools of Buddhism can also be seen in their respective history. In Ceylon, there has been practically but one school ever since the introduction of Buddhism there. The Singhalese Buddhists have had one code of morality, the Vinaya, which is recorded in detail in their scriptures, and which, being so very explicit in its enunciation that even the uncultured could comprehend it readily, does not allow of very widely divergent interpretations. Accordingly there were few chances for dissension. The Vinaya as it is practised to-day in Ceylon has not changed even in its details since the day of its first promulgation there. In this respect we can say that Hînayâna Buddhism faithfully preserves the practical form of Buddhist moral culture as it developed during the time that elapsed after the decease of the Buddha down to the despatch of the Asoka missionaries to this district. I emphasize this latter point; for it is quite reasonable to suppose, and the supposition is justified by the records in our possession, that Buddhism began to grow in its diverse aspects soon after the death of the Buddha.

History, however, shows us a very different state of affairs among the Mahâyâna followers. Into how many schools did the Mahâyâna divide itself! And how vehemently did each school defend its own doctrine against the others! While the Hînayâna Buddhists evidently kept quiet, the Mahâyâna spoke disparagingly of their rival believers, and this was altogether unworthy of their professed liberalism. In fact, it was through their self-conceit that they came to designate themselves as Mahâyâna Buddhists, followers of the Great Vehicle of Salvation, which had in view the discrediting of their conservative brethren in the faith. This spirit of self-exaltation was exhibited not only against the more orthodox ethical adherents of Buddhism, but also among themselves, as witness the famous founder of the Nichiren or the Pundarîka sect of Buddhism in Japan. His denunciation of the other Buddhist sects then existent in Japan was so strong and abusive that the authorities of the time thought it politic to get rid of him quietly, though I must add that his prosecution was not solely due to religious reasons.

This struggle and fighting, however, was quite in accord with the somewhat one-sided development of the Mahâyâna in the direction of speculative philosophy. Intellect is always inclined to dissent, to quarrel, to become self-conceited. The existence of ten or twelve sects of Japanese Buddhism was the inevitable result of the general movement of the speculative Mahâyâna. Of course, the other phases of Buddhism were not altogether forgotten; for the practice of *dhyâna* (meditation) is still in evidence,—indeed there is one sect in Japan and China bearing its very name and exercising much influence, especially among the educated classes. However this may be, the fact remains that the Mahâyâna is a development of one side—the intellectual, speculative, philosophical side—of Buddhism, while the Hînayâna preserves the ethical side of Buddhism

comparatively in its pure and primitive form. To realize the perfect type of Buddhism, the threefold treasure, Triratna, must be equally developed; the Buddha, the Dharma and the Sangha must stand side by side imbued with the same spirit as when they were first established, whatever outward transformation they might have undergone according to varying circumstances. If the Hînayâna is said to have the Sangha in its model form, the Mahâyâna may be considered to have fully developed the Dharma, that is, the religio-philosophical signification of Buddhism; while both schools claim the Buddha as their common founder. The problem that faces faithful Buddhists at present is how best to effect a complete reconciliation of the moral discipline of Hînayâna with the speculations of Mahâyâna.

Now let us see how Mahâyâna Buddhism has developed its speculative course as compared to the Hînayâna, and I will first discuss the doctrine of *anâtman,* or non-ego. This is considered one of the most important and characteristic features of Buddhism, and justly so, because both the Hînayâna and the Mahâyâna uphold this as essential to their *raison d'être.* However, the Hînayâna school seems to have remained almost too faithful, as it were, to the doctrine; it has not gone beyond its negative statement; it has not carried out its logical consequence to the utmost limits. On the other hand the Mahâyâna has not only extended the theory from its subjective significance to the objective world, but has also boldly developed the positive conclusion implied in it. I do not mean that the Hînayâna has none of these tendencies as shown by the Mahâyâna; in fact, it seems to contain everything Mahâyânistic in germinal form. What most eminently distinguishes the Mahâyâna school in this connection is that it makes the most explicit, manifest, unequivocal, and fearless assertions on these religio-philosophical questions which deeply concern the human heart.

With regard to the non-ego theory, the Mahâyâna followers assert that there is no *âtman* or ego-soul not only in its psychological signification, but in its objective sense. That is to say, they deny with the Hînayâna followers that there is any such thing as an ego-entity, a concrete, simple, ultimate, and independent unit, behind our consciousness; but they go still further and declare that this objective world too has no *âtman*, no ego, no personal creator, no Ishvâra, who works and enjoys his absolute transcendence behind the eternal concatenation of cause and effect. This is technically known as a double negation of the subjective and the objective world, and it is on this account that the Mahâyâna school has often been called, though unjustifiably and quite incorrectly, nihilism or *çûnyavâdin*.

In this connection, it may be of interest to quote a Western Buddhist scholar's opinion of Buddhism as typical of a prejudiced and uncritical judge. Eitel, a noted scholar of Chinese Buddhism, speaks thus of the Buddhist doctrine of Nirvana in his *Three Lectures on Buddhism*, which were delivered in Union Church, Hongkong, 1870-71: "Nirvâna is to them [the Buddhists] a state of which nothing could be said, to which no attributes can be given; it is altogether an abstract, devoid alike of all positive and all negative qualities. What shall we say of such empty, useless speculations, such sickly, dead words, whose fruitless sophistry offers to that natural yearning of the human heart after an eternal rest nothing better than a philosophical myth? It is but natural that a religion which started with moral and intellectual bankruptcy should end in moral and intellectual suicide" (p. 21, column 2).

As a matter of fact, the Mahâyâna followers do not regard negation as the ultimate goal of their speculations. With them negation is but a road to reach a higher form of affirmation; for they are aware of the fact that the

human mind lives in affirmation and not in negation. Any critic of Mahâyâna philosophy, who with sympathetic insight penetrates deep enough into its heart, would readily find that behind a series of negations offered by the Mahâyâna thinkers there is really the assertion of a higher truth, which, owing to the limitations of the human intellect, cannot be presented by any other means than by negation. It is not due to sophistry nor mere abstraction that the Buddhists sometimes appear to delight in a negative statement of truth. They are most earnestly religious; they know that the deepest religious truth cannot be presented in a stereotyped philosophical formula. Only those who are short-sighted timidly stop at the negation and refuse to go beyond. If they thus misjudge the significance of Mahâyâna Buddhism, the fault is on their own side.

What, then, is that positive something offered by the Mahâyâna scholars as the logical conclusion of the theory of non-atman? It is generally designated as *tatvâ* or suchness. This is a philosophical term, and when its religious importance is emphasized it is called *dharmakâya.*[1] In this conception of suchness, or *dharmakâya,* they find the highest possible affirmation which is reached after a series of negations and which unifies all forms of contradiction, psychological, ethical, and ontological. Ashvagosha, one of the greatest early Buddhist philosophers in India, says in his *Awakening of Faith in the Mahâyâna*:[2] "Suchness is neither that which is existent, nor that which is non-existent; it is neither that which is at once existent and non-existent, nor that which is not at once existent and

[1] The term *dharmakâya* is very difficult to define. Neither "essence-body," "law-body," nor "being-system" exactly expresses its idea. "Dharma" is a very comprehensive term in Buddhist philosophy, and in this case it means all this: essence, being, law, and doctrine. In short, let us understand "Dharmakâya" here as the source, the ultimate reality, from which is derived the reason of existence, morality, and religion.

[2] Translated into English by the present author from the Chinese translations, 1900. A new revised edition will be issued in the near future.

non-existent; neither that which is one, nor that which is many; neither that which is at once one and many, nor that which is not at once one and many....It is altogether beyond the conception of a finite mind, and the best way of defining it is to designate it 'suchness.' "

Nâgârjuna, the founder of the Mâdhyamika school of Buddhism in India, who was as great as Ashvaghosha, declares in his *Book of the Mean*: "No birth, no death, no persistence, no changing, no oneness, no manyness, no coming, no going: this is the doctrine of the mean." Again, "To think 'it is' is eternalism: to think 'it is not' is nihilism. To be or not to be, the wise cling to neither."

All these statements have been construed as nihilistic, leading the mind nowhere but to absolute emptiness. But, as I have said before, such critics entirely ignore the fact that the human understanding, owing to its constitutional limitations, often finds it most expedient and indeed most logical to state a truth negatively, since a negation is really a higher form of affirmation, to be comprehended only through a process of intuition. The Mahâyâna thinkers have denied with their conservative fellow believers the existence of a concrete ego-soul; they have refused to accept the doctrine of a personal God; they are further reluctant to assert anything dogmatically; and the ultimate logical sequence of all these necessarily negative statements could not be anything else but the conception of suchness. Beyond this, one enters into mysticism; philosophy must bow her head modestly to religion at this gate of suchness; and religion must proceed by herself into an unknown wilderness, or to Eckhart's *stille Düsterniss* or *Wüste,* or to Boehme's *Abgrund*;—this is the realm of "Eternal Yea," or, which is the same thing, the realm of "Eternal Nay." The Mahâyâna philosophy at this point becomes mysticism. Intellectually, it has gone as far as it can. *Vidya* must now give way to *dhyâna* or

prajñâ; that is, intellection must become intuition, which is after all the ultimate form of all religious discipline. Mysticism is the life of religion. Without it religion loses her reason of existence; all her warm vitality departs, all her inexpressible charm vanishes, and there remains nothing but the crumbling bones and the cold ashes of death. I have said before that the Mahâyâna is highly speculative, but I must add that it is also most deeply and thoroughly religious.

It is apparent that with the conception of suchness, the Mahâyâna speculations have reached the highest peak, and upon this summit stands the religious edifice of Mahâyâna Buddhism. Superficially, the Mahâyâna seems widely different from the Hînayâna; but when its development is traced along the lines indicated above, one will readily comprehend the fact that in spite of the disparity existing between the two *yânas* of Buddhism, the Mahâyâna which started intellectually and culminated in mysticism, as every religion should, is really no more than a continuation of the Hinayâna.

When the conception of suchness is established, the *raison d'être* of the Mahâyâna becomes manifest. Buddhism is not an agnostic system of philosophy, nor is it an atheistic ethics. For in suchness, or *dharmakâya*, it finds the reason of existence, the real reality, the norm of morality, the source of love and goodness, the fountainhead of righteousness, the absolute intelligence, and the starting-point of *karma*, the law of deeds. For suchness, according to Mahâyâna thinkers, is not merely a state of being, but it is energy, intelligence, and love. But as suchness begins to assume these attributes, it ceases to be transcendental suchness, it is now conditional suchness. So long as it remains absolutely transcendental, in which neither negation nor affirmation is possible, it is beyond the comprehension of the human understanding; suchness can not

very well become the object of our religious consciousness. But there is the awakening of the will in suchness, and with this awakening we have conditional and self-limiting suchness in place of the absolute unknowable. As to the reason and manner of this process the human mind has to confess a profound and eternal ignorance. It is in this transformation of suchness that the Mahâyâna system perceives the religious significance of *dharmakâya.*

The *dharmakâya* is now conceived by the human heart as love and wisdom, and its eternal prayer is heard to be the deliverance of the ignorant from their self-created evil *karma* which haunts them as an eternal curse. The process of deliverance is to awaken in the mind of the ignorant the *samyaksambodhi,* the most perfect wisdom, which is a reflection in sentient beings of the *dharmakâya.* This wisdom, this *bodhi,* is generally found asleep in the benighted, because a sort of spiritual slumber results from the narcotic influence of the evil *karma,* which has been and is being committed through the non-realization of the presence of the *dharmakâya.* Deliverance or enlightenment consists, therefore, in making every sentient being open his spiritual eye to this fact. It is not his ego-soul that makes him think, feel, desire, or aspire, but the *dharmakâya* itself in the form of *bodhichitta* or wisdom-heart which constitutes his ethical and religious being. If we abandon the thought of egoism and return to the universal source of love and wisdom, we are released from the bond of evil *karma,* and we are enlightened as to the reason of existence; in short, we are Buddhas.

In trying to make a sentient being realize the presence in himself of the *bodhichitta,* the *dharmakâya* can be said to be working for its own sake, that is, to awake from the spell of ignorance. Here is involved a great philosophical and religious problem. In the beginning, the *dharmakâya*

negated itself by its own affirmation, and it is now working to release itself from the negation through which this world of particulars was created. This is, as it seems to our limited intellect, an eternal process of suchness: from affirmation to negation and from negation to affirmation. To this mystery of mysteries, however, we fail to apply our rules of syllogism; we have simply to state the fact that, though it seems apparently contradictory, our religious consciousness finds in this mystery something unspeakably fascinating and indeed the justification of its eternal yearnings.

As a consequence of the conception of *dharmakâya* as eternal motherhood and as the source of infinite love, the doctrine of *karma* had somehow to modify its irrefragable severity. And here we observe another phase of differentiation as effected by the Mahâyâna Buddhists from the doctrine commonly held by their ethical, monastic Hînayâna brethren. I do not maintain that the doctrine of *karma* is denied by the Mahâyâna followers. Far from it. They adhere to the doctrine as firmly as the Hînayâna philosophers; they have taken away from it only its crushing effects upon the sinful, who are always too weak and too timid to bear the curse of all their former evil deeds. Or, in other words, the Mahâyâna Buddhists offer a doctrine complementary to that of *karma* in order to give a more satisfying and humane solution to our inmost religious needs. The Mahâyâna doctrine of *parinâmanâ*, therefore, must go side by side with that of karma; for through this harmonious co-working of the two, the true spirit of Buddhism will be more effectively realized.

The doctrine of *parinâmanâ* is essentially that of vicarious sacrifice. Apparently, it contradicts the continuity of the action of *karma*; but in Mahâyâna Buddhism *karma* is conceived in its cosmic rather than in its individualistic aspect, which makes it possible to reconcile the

two notions, *karma* and *parinâmanâ*. I will try to make this clear.

First, what does *parinâmanâ* mean? It means "to bend," "to turn about," or "to deliver," or "to transfer," or "to renounce," for which the early Chinese Buddhists have *hui hsiang*, which means "to revolve and be directed towards," that is to say, "to turn a thing about and hand it over to another." The doctrine of *parinâmanâ*, then, is to turn one's merits over to another, to renounce oneself for others, to sacrifice oneself for the sake of others, to atone for others' evil *karma* by one's own good deeds, to substitute oneself for another who according to the law of *karma* ought to suffer himself. Or, to use Christian terminology, the doctrine of *parinâmanâ* is in its principle that of vicarious sacrifice; with this difference, however, that while in Christianity vicarious sacrifice means the death of Christ on the cross for the sins of all mankind, the Mahâyâna philosophy does not confine the principle of vicarious sacrifice to a solitary historical incident. Christianity is built upon the history of a person, whatever its intrinsic authenticity may be, and not directly upon the facts of our religious consciousness and intellectual necessity. Therefore, it is unable to uphold the universal application of the principle of vicarious sacrifice, not to say its inability to appreciate the importance of the principle of *karma*. This is where Christianity derives its strength, the strength of concreteness and objectivity, as compared to Mahâyâna Buddhism; but here lies also its weakness, at least so it would seem to Buddhist thinkers.

The notion of *parinâmanâ* is based upon the following truths: The universe, according to the Mahâyâna, is a grand spiritual system composed of moral beings, who are so many fragmentary reflexes of the *dharmakâya*. The system is so closely knitted together that when any part of it or any unit composing it is affected in one way

or another, good or bad, all the other parts or units must be drawn into the general commotion that would follow, and will share the common fate. This subtle spiritual system of which all sentient beings are parts or units, is like a vast ocean in which the eternal moonlight of *dharmakâya* is reflected. Even a faint wavelet which is noticed in one part of the water is sure to spread all over its surface sooner or later according to the resistance of the molecules, and thus finally to disturb the serenity of the lunar image in it. Likewise, with every deed, good or bad, committed by any sentient unit of this spiritual organization, the *dharmakâya* rejoices or is grieved. When it is grieved, it wills to counteract the evil with goodness; when it rejoices, it knows that so far the cause of goodness has been advanced. Individual *karma,* therefore, is not after all individual; it is most intimately connected with the whole. It is not an isolated phenomenon originating from the individual and returning to the same agent. It is no mere abstraction when I say that the lifting of my arm or the moving of my leg is not an accidental, indifferent act, but directly related to the ultimate cause of the universe.

This assertion appears with an immeasurably greater emphasis with reference to an act which has a moral bearing. "If," we may ask, "in our spiritual plane of existence things are so intimately related to one another, why could we not make the merit of our own deeds compensate or destroy the effect of an evil *karma* created by an ignorant mind? Why could we suffer ourselves for the sake of others and lighten even to the smallest degree the burden of evil *karma* under which weak, ignorant ones are groaning, though they have nobody else to blame for their own wretchedness?" These questions are answered affirmatively by the Mahâyâna Buddhists. For they say: "It is possible for us to dedicate our own good *karma* to the cause of universal goodness, and to suppress or crush or to make

quite inefficacious the evil *karma* perpetrated by the ignorant. It is possible for us to substitute ourselves for others and to bear their burden upon our own shoulders in order to save them from their self-created curse." The result of this conviction is the doctrine of *parinâmanâ*.

In this, however, it is seen that quite in accordance with the cosmic conception of *dharmakâya,* the Mahâyâna philosophers emphasize the universal or supra-individual significance of *karma* more than its solitary, individual character. In the Hinayâna system, the conception of *karma* is individualistic, pure and simple; there is no escape whatever from the consequence of one's own evil or good deeds, for it follows one even after death which is merely another form of birth. The Mahâyâna Buddhists believe in this as far as the law of causation is concerned; but they go a step further, and assert that *karma* also has its cosmic or supra-individual aspect, which must be taken into consideration, when we want to realize fully the meaning of our spiritual existence. Though a man has to reap what he has sown and there is no escape possible from the consequence of his evil deeds, the Mahâyâna thinkers would say: A Bodhisatva wishes from the fulness of his heart to turn over whatever merit he can have from his acts of goodness to the general welfare of his spiritual kingdom, and to bear upon himself whatever burden of evil is going to befall his ignorant, self-destroying fellowbeings. The good he does is not necessarily for his own benefit. In whatever deed he performs, he does not forget its universal character; above all, he desires to be of service in any capacity whatever to the whole spiritual organization, of which he is a unit.

Therefore, the doctrine of *parinâmanâ* is no more than that of vicarious sacrifice. It is in point of fact vicarious sacrifice in that a Bodhisatva wishes to bear the burden of evil for the real offenders and to save them from suffer-

ing. But when he works to add to the general "stock of goodness" and to nourish the "root of merit" in this world, he is doing more than mere substituting; he is doing something positive. *Parinâmanâ* is vicarious sacrifice, self-renunciation, the transference of merit, the promotion of universal goodness, the annihilation of me and thee, the recognition of the oneness of all things, and the complete satisfaction of our inmost religious yearnings.

The doctrine of *karma* is terrible, the doctrine of *parinâmanâ* is humane: *karma* is the law of nature, inflexible and irreconcilable; *parinâmanâ* is the heart of a religious being, filled with tears: the one is rigidly masculine and knows no mercy whatever; the other is most tenderly feminine, always ready to weep and help: the one is justice incarnate; the other is absolute compassion: the one is the god of thunder and lightning, who crushes everything that dares resist him; the other is a gentle spring shower, warm, soft, and relaxing, and helping life to grow: we bow before the one in awe and reverence; we embrace the other as if finding again our lost mother; we need the one, for we must be responsible to the *dharmakâya* for our thoughts, feelings, and deeds; but we cannot let the other go as we need compassion, tolerance, humaneness, and lovingkindness. Mahâyâna Buddhism can thus be said to have a singularly softening effect on the conception of *karma*. *Karma* cannot be denied, it is the law; but the human heart is tender and loving. It cannot remain calm and unconcerned at the sight of suffering, however this might have been brought about. It knows that all things ultimately come from the same source. When others suffer I suffer too; why then should not self-renunciation somehow moderate the austerity of *karma*? This is the position taken by the Mahâyâna Buddhists in regard to the doctrine of *karma*.

With the moderation of the principle of *karma*, there

took place another change in the Mahâyâna concerning the ideal man, that is, as to what constitutes the true ideal Buddhist, or what kind of being he must be who really embodies all the noble thoughts and enlightened sentiments of Mahâyâna Buddhism. Arhatship was not satisfactory in this respect, and ceased to be the goal of religious discipline for the followers of the Mahâyâna. They considered the Arhat as not fully realizing all the inmost aspirations of religious consciousness; for is he not a Buddhist who seeks only his own deliverance from the whirlpool of birth and death, in which all beings are struggling and being drowned? So long as *karma* was understood in its individualistic aspect, Arhatship was quite the right thing for Buddhists to aspire after; but *karma* could be interpreted in another and a wider sense, which made the doctrine of *parinâmanâ* possible, and the Mâhayâna Buddhists thought that this was more in accord with the deepest yearnings of a religious being who wants to save not only himself but the entire world as well. Therefore, the speculative Buddhists came to establish the ideal of Bodhisatvahood in place of Arhatship, and for this reason the Mahâyâna is often designated as Bodhisatvayâna in contradistinction to Shrâvakayâna and Pratyekabuddhayâna. (*Yâna* means a vehicle on which sentient beings are carried from this shore of ignorance to the other shore of enlightenment and eternal happiness.)

The development of this ideal of Bodhisatvahood was quite natural with the Mahâyâna Buddhists. Grant that the Hînayâna followers most faithfully adhered to the moral, monastic, and disciplinary life of primitive Buddhism, and that the Mahâyânists in the meantime were bent on the unfolding of the religio-philosophical significance of the teachings of the Buddha, and it will be seen that the further they advanced the wider grew their separation from each other. To the moralists, such a bold flight

of imagination as conceived by the Mahâyâna was a very difficult thing to realize. Moral responsibility implies a strict observance of the law of *karma*; what is done cannot be undone. Good or bad, one has to suffer its consequence; for nobody can interfere with it. Arhatship alone, therefore, could be made the goal of those self-disciplining moralists. With the Mahâyâna Buddhists, however, it was different. They came to look at the import of our moral action more from the point of its cosmic relations, or from that of the most intimate inter-dependence that obtains among all sentient beings in their moral, intellectual, and spiritual activities. With this change of the point of view, they could not but come to the conception of a Bodhisatva whose religion was the realization of the doctrine of *parinâmanâ*.

In point of fact, there are not two Buddhisms. The Mahâyâna and the Hînayâna are one; the same spirit of the founder of Buddhism breathes through both. Only each has developed in its own way, according to the different surroundings in which it has thrived and grown—understanding by surroundings all those various factors of life that make up the peculiarities of an individual or a nation. Lack of communication has hitherto prevented the bringing together of Buddhists and the effecting of a complete understanding of each other. But the time is coming nearer when each will fully realize and candidly admit its own shortcomings, though not oblivious of its advantages, and earnestly desire to cooperate with the other in order to bring about a perfect assimiliation into one uniform system of Buddhist thought and Buddhist practice, and to contribute to the promotion of peace and goodwill towards all beings, regardless of racial or national differences.

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DEFINITIONS AND METHODOLOGICAL PRINCIPLES IN THEORY OF KNOWLEDGE.

IN the following article, I propose to suggest some definitions resulting from the analysis of experience suggested in previous articles, and some methodological precepts which I believe to be useful in the later portions of theory of knowledge. It was scarcely possible to state either the definitions or the precepts until we had decided that the relation of acquaintance is fundamental to experience, for almost all definitions and principles that we can adopt will presuppose this theory.

Subjects have been already defined in a previous article as entities which are acquainted with something, and *objects* as entities with which something is acquainted. In other words, subjects are the domain, and objects are the converse domain, of the relation *acquaintance*. It is not necessary to assume that acquaintance is unanalyzable, or that subjects must be simple; it may be found that a further analysis of both is possible. But I have no analysis to suggest, and therefore formally both will appear as if they were simple, though nothing will be falsified if they are found to be not simple.

Cognitive facts will be defined as facts involving acquaintance or some relation which presupposes acquaintance.

It is to be observed that we can define cognitive *facts*, not mental *particulars*, since we have no reason to assume

that subjects are in fact always or ever of a different kind from other particulars. The distinguishing mark of what is mental, or at any rate of what is cognitive, is not to be found in the particulars involved, but only in the nature of the relations between them. Of these relations acquaintance appears to be the most comprehensive and therefore the most suitable for the definition of cognitive facts.

There is, however, a certain difficulty in defining the word "presuppose" which occurs in the above definition. What is intended is to include such relations as (e. g.) attention and judgment, which seem plainly to involve acquaintance with the object attended to or the objects entering into the judgment. But it is not intended to include relations which merely *happen,* as an empirical fact, to occur only where there is acquaintance. In the cases which it is intended to include, the fact that acquaintance is involved seems *a priori* evident, and not merely a statistical occurrence. I do not know what, precisely, is the logical difference between these two kinds of connection, but it seems plain that there is a difference. The kind of connection intended in the definition is what we may call the *a priori* kind; and since the difficulties of the distinction belong solely to logic, we need not investigate them further at present.

Theory of knowledge, or *epistemology*, is more difficult to define. To begin with, no definition can be satisfactory which introduces the word "knowledge," both because this word is highly ambiguous, and because every one of its possible meanings can only be made clear after much epistemological discussion. Then again, difficulties arise as regards the relations of epistemology to psychology and to logic respectively. It is obvious that much of epistemology is included in psychology. The analysis of experience, the distinctions between sensation, imagination, memory, attention, etc., the nature of belief or judgment, in

short all the analytic portion of the subject, in so far as it does not introduce the distinction between truth and falsehood, must, I think, be regarded as strictly part of psychology. On the other hand, the distinction between truth and falsehood, which is plainly relevant to the theory of *knowledge*, would seem to belong to logic, though this is open to some degree of doubt. And in any case, as soon as we reach the theory of judgment, even apart from truth and falsehood, the difficulties encountered are almost entirely logical, and logical discoveries are what are most required for the progress of the subject. It can be shown[1] that a judgment, and generally all thought whose expression involves *propositions*, must be a fact of a different logical form from any of the series: subject-predicate facts, dual relations, triple relations, etc. In this way, a difficult and interesting problem of pure logic arises, namely the problem of enlarging the inventory of logical forms so as to include forms appropriate to the facts of epistemology. It would seem, therefore, that it is impossible to assign to the theory of knowledge a province distinct from that of logic and psychology. Any attempt to mark out such a province must, I believe, be artificial and therefore harmful.

The central problem of epistemology is the problem of distinguishing between true and false beliefs, and of finding, in as many regions as possible, criteria of true belief within those regions. This problem takes us, through the analysis of belief and its presuppositions, into psychology and the enumeration of cognitive relations, while it takes us into logic through the distinction of truth and falsehood, which is irrelevant in a merely psychological discussion of belief. We may define epistemology in terms of this problem, as: *The analysis of true and false belief and their presuppositions, together with the search for criteria of true*

[1] As I have come to know through unpublished work of my friend Mr. Ludwig Wittgenstein.

belief. But *practically* this definition is somewhat wide, since it will include parts of psychology and logic whose importance is not mainly epistemological; and for this reason, the definition must not be interpreted quite strictly.

Data are by no means easy to define, since there are various logically different kinds of data. The simplest kind will be *objects*: in this sense, a given person's data are those *particulars* with which he is acquainted. Even in this very narrow sense, there are, as I shall try to show in subsequent articles, still at least three ways in which data may be given, namely in sense, in memory, or in imagination; but these different ways of being given do not involve corresponding differences in the objects given. On the other hand, we can understand universals, and any primitive understanding of a universal (i. e., any understanding not derived from some other understanding) is in a sense a case of acquaintance, and is certainly sufficient ground for calling the universal in question a datum. What is the correct analysis of the understanding of universals is a difficult question, but such understanding must certainly be included among the data of knowledge. Again, *perceptions of facts* (as opposed to the corresponding judgments) must be included among data; if I see that one thing is to the left of another, or if I observe that the thunder is later than the lightning in a particular case, these perceived facts are certainly among my data. And perceived facts are not always thus particular: general logical facts, for example, are often such as can be perceived. I do not know whether any *judgments* (as opposed to perceptions) are to be included among the data of knowledge: arguments may be adduced on either side in this question. But certainly all epistemologically legitimate inference demands that both the premise and the connection of premise and conclusion should be data, either for perception or for judgment.

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I think we may say broadly that *data* include all particulars, universals, and facts, which are cognized otherwise than by inference or by belief not derived from analysis of a perceived fact. Thus the only cognitions which do not give data are (*a*) beliefs obtained by inference, and (*b*) beliefs not obtained either by inference or by analysis of perceived facts. Beliefs of the second kind might be fitly called *prejudices*: it will be a mere accident whether they are true or false. Inferred beliefs, on the other hand, if derived from data and not from prejudices, will be true. But in these remarks we are anticipating the outcome of lengthy discussions which cannot be undertaken at present.

There is another, somewhat vaguer, sense in which the word "data" may be used in epistemology. If we are analyzing some body of knowledge, say physics, with a view to disentangling its truth and falsehood, we may give the name "data" to all the beliefs which we entertain when we enter on the inquiry. In this sense, "data" will be opposed to "premises," and what we have hitherto called data will be more fitly called premises. This usage is convenient in some discussions, but for our present purposes it seems not desirable.

It is a common error to suppose that it must be obvious what are our data in any subject-matter to which we turn our attention. This need by no means be the case, since something may quite well be a datum without the fact of its being a datum being also a datum. There is therefore nothing surprising in the fact that it is often very difficult to discover what our data really are.

I pass now to some methodological principles which may be useful in our subsequent inquiries.

1. *The objects of acquaintance cannot be "illusory" or "unreal."* It is customary to speak of dreams and hallucinations as illusory, and to regard images as unreal in some sense not applicable to the objects of normal sensa-

tions. No doubt there are important differences between the objects commonly considered "real" and the objects commonly considered "unreal." But for purely logical reasons, these differences demand some entirely different analysis. If an object is given in acquaintance, then that object has a certain relation to the subject which is acquainted with it. But this would be meaningless if there were no such object. An object of acquaintance is an object to which it is possible to give a proper name, as opposed to a description; it may become the "this" of attention. Of such an object, it is meaningless to suppose it unreal. A proper name which names nothing is not a proper name, but a meaningless noise. An acquaintance which is acquainted with nothing is not an acquaintance, but a mere absurdity. The conception "unreal" is not applicable to such immediate data, but only to *described* entities. We may say "the present king of France is unreal," meaning that there is no present king of France, but not meaning "there is an entity which is at present king of France and which is unreal." Unreality, in fact, is a notion which owes its origin to grammatical forms. These forms make it possible for the grammatical subject of a sentence to be a description, which is not the name of any constituent of the proposition expressed by the sentence. In such a case, if the description applies to nothing, we may say that the subject is unreal; and if we fail to realize that the unreal subject is merely grammatical, we may come to think that there are entities which are unreal. This, however, is a plain absurdity: given an entity, it is absurd to inquire whether it is real or unreal: there the entity is, and there is no more to be said. Thus the supposition that some objects with which we are acquainted are unreal is one which logic shows to be untenable.

The conclusion that no object of acquaintance can be "unreal" forces upon us a certain attitude of respect

towards dreams, hallucinations, and images. There are, of course, important epistemological differences between such objects and the objects of normal sensation, but these differences belong to a much later and more complex part of the subject. Their *likeness* to the objects of normal sensation is much less realized, but is far more important for our present purpose. Mankind has gradually selected a kind of aristocracy among entities, which it calls the "real" world. But, as Parmenides told Socrates, in philosophy we must not despise even the meanest things; and the entities which mankind has condemned as "unreal" are full of interest and importance. It will be said: "No doubt the things we see in dreams exist in a sense, but it is a purely subjective sense: they exist only because we are dreaming, and are merely parts of our mind." This may or may not be true. I am not concerned at present with the question whether it is true or false. I wish merely to point out the great complexity of what is asserted, and the immense system of knowledge which it presupposes. It is asserted that the objects of dreams exist only because we are dreaming; this means, I suppose, that there is some essential impossibility in such objects existing except when some subject is aware of them. It is asserted that they exist only in our mind; this means, I suppose, that not more than one subject can possibly be aware of them. But to know all this is to have an enormous knowledge of the external world, and to see that it cannot harbor anywhere such objects as I saw in my dream. And there is always the fear that the same proposition might be maintained regarding the things seen in waking life. If dreams are to be given a lower status than waking life, as a source of knowledge concerning the external world, this must be done, not by condemning them *ab initio,* but by showing that the world which is constructed on the assumption that they and waking life have exactly the same *immediate*

reality is one in which dream-objects are less intimately related to *other* entities than are the objects of our waking sensations. In fact, what is *called* the unreality of an immediate object must always be really the unreality of some other object inferred from the immediate object and described by reference to it. This conclusion, we shall find, is of great importance in all problems concerning our knowledge of the external world.

2. *The possibility of error in any cognitive occurrence shows that the occurrence is not an instance of a dual relation.* This maxim is closely connected with our first maxim, and equally has a purely logical origin. We saw that when we are acquainted with an object, there certainly is such an object, and the possibility of error is logically excluded. Exactly the same argument would apply to any two-term relation. But when the occurrence is one which unites the subject with several objects in a single fact, the situation is different. In this case, the several objects may or may not be themselves the constituents of a certain related fact; thus a certain described fact, namely the fact composed, in some specified manner, of the objects of the occurrence in question, may occur in certain cases and not in others. The further explanation and exemplification of this maxim will come better in the account of belief. For the present, its chief importance is negative: where error is possible, something not acquaintance or attention or any other two-term relation is involved. This applies, for instance, to the case of memory, in so far as memory is fallible.

3. *The epistemological order of deduction involves both logical and psychological considerations.* It is obvious that any order of valid deduction must involve *logical* considerations, since the propositions deduced must follow logically from their premises. But given a certain body of propositions of which some are deducible from others, there will be many ways of selecting some among them as

the premises from which the others are to be inferred. Any such set of premises may be called a set of *logical premises* for that body of propositions. In a purely logical problem, in which we do not consider the question whether any of the body of propositions are *known to be true,* conditions of elegance suggest that a set of logical premises should be such that no one follows from the others, and should consist of the smallest possible number of the simplest possible propositions. In epistemology, however, our premises have to fulfil other requisites. We are concerned not merely with truth, but with knowledge; hence our premises should be such as can be known to be true without being deduced from any others of the body of propositions in question. For this reason, a certain artificial naïveté is required in beginning epistemology: we must avoid assuming many things which we firmly believe to be true, but which can only be reached by a process of inference. On the other hand, in order to minimize the risk of error, which always exists in regard to premises, it is wise to include whatever appears to be known without proof, even if it could be deduced from other propositions so known. Thus the set of *epistemological premises* for any body of propositions consists of all those relevant propositions which appear to be known otherwise than by inference, and from which the given body of propositions can be deduced. If some of the epistemological premises can be deduced from others, that tends to confirm our belief in the premises, and does not afford a reason for excluding those that can be deduced. We may call a body of propositions *epistemologically self-contained* when it contains all its own epistemological premises. The propositions constituting our epistemology must, of course, be epistemologically self-contained—except that some of its premises belong to logic; but those that belong to logic ought to be stated. Since the question as to what appears to be known without

inference involves psychology, the epistemological order is partly determined by psychological considerations. The real business of epistemology is largely the discovery of its own epistemological premises; the deductions from them will of course proceed according to the rules of logic, and may therefore be left to the logician.

4. *A knowledge of physics and physiology must not be assumed in theory of knowledge.* This maxim follows from the preceding account of the epistemological order. Physics and physiology belong to our knowledge of what is called the external world; our knowledge of them is obviously dependent upon sensation, and is obtained by methods which the theory of knowledge must investigate. Thus however firmly we may be convinced of the truth of physics and physiology, we must only use this conviction as a means of *testing* our epistemology. not as affording premises upon which our epistemology may build from the start. To take a crude illustration: if we are considering whether or how the sense of sight gives knowledge of physical objects, we must not assume that we know all about the retina, for the retina is a physical object of which we obtain knowledge by seeing it. Thus to assume that we know this or that about the retina is to assume that we have already solved the epistemological problem of the physical knowledge to be derived through sight.

It is to be observed, however, that it would not be a logical fallacy to assume knowledge of the organs of sense for a purely sceptical purpose, i. e., in order to show that no knowledge of physical objects is to be derived from the senses. If it could be shown that the hypothesis that we obtain such knowledge through the senses leads to a theory of the sense-organs which shows that they cannot give such knowledge, then the hypothesis with which we started would have refuted itself, and would therefore be false. But although some such position is sometimes advanced,

and is not *logically* unsound, it is hardly possible that it should be true in substance. The hypothesis that we obtain knowledge of physical objects through the senses is one which is capable of a multitude of forms, according to the view we adopt as to the nature of physical objects. If one view on this point is self-refuting, another may not be, and we can never be sure that we have tried all possible views. Thus a dogmatic scepticism as to the knowledge of physical objects derived from the senses can never be warranted by such arguments as we have been considering. Nevertheless, the possibility is one which it is important to remember, since it affords a test to which any theory as to our knowledge of the external world may be submitted.

It is no doubt the case that our sensations are functions of the sense-organs and the nervous system, but this is not the *primitive* knowledge—it is a scientific inference, and cannot form part of the epistemological premises of epistemology. In epistemology, it is important to reduce our problems to what is actually experienced. Take, for example, such a simple fact as that we no longer see objects when we shut our eyes. A number of immediate experiences have to be combined in order to reach this result. We can know from our own experience that visual objects disappear when we have certain muscular and tactile sensations, which we learn to locate in our eyelids. By shutting one eye in front of a looking-glass we may learn (assuming what we know of looking-glasses) what is the visual appearance of the eyelid when we have the muscular sensation in question. By seeing others shut their eyes, and learning that then they no longer see, we can learn without looking-glasses what shutting the eyes looks like. But all this is a complicated process, learned, no doubt, at an early age, but not known without learning. Thus the dependence of sight upon the eyes is not the sort of thing to be *assumed* in epistemology. And what is true of this

very simple physiological fact is true *a fortiori* of the complicated and difficult knowledge concerning the brain which is sometimes thought relevant to the foundations of epistemology. All such knowledge is excluded by the rule which must guide us throughout our investigation: Seek always for what is obvious, and accept nothing else except as the result of an inference from something obvious which has been found previously. With this rule, a great simplification is effected, and vast masses of erudition can be swept aside as irrelevant.

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CRITICISMS AND DISCUSSIONS.

THE PROBABLE ORIGIN OF OENOTHERA LAMARCKIANA SER.*

In a series of most interesting articles, B. M. Davis has recently tried to prove that mutability might be a result of previous crosses. This view was first proposed by Bateson and Saunders, and applies especially to the phenomena which *Oenothera Lamarckiana* shows when seeds from the pure strain, and even from pure lines within this strain, are sown, as in the experiments I conducted in my experimental garden. Davis expected to be able to offer the desired proof by showing that *O. Lamarckiana* might be duplicated by crossing two other species of the same group. Up to this time, as a matter of fact, he has not succeeded in producing any form which comes sufficiently near *O. Lamarckiana* to be compared with it.[1] But if he had succeeded in doing so, evidently it would not have been a proof for his assertion, unless his hybrid should show the same degree of mutability as does *O. Lamarckiana*, since we have as yet no means of judging from the morphological characters of a given plant whether its hereditary characters are in a stable or in an unstable condition.

In starting his experiments to produce a duplication of Lamarck's evening primrose, Davis was unfortunate in the choice of species for his combination. He chose *O. biennis* L. and a form which he assumed to be *O. grandiflora* Aiton. It is evident that the first condition of success in such work consists in the purity and the immutability of the species which are to produce the hybrid. If they are already in a mutable condition, it is to be

* Reprinted from the *Botanical Gazette* of April, 1914.

[1] For a successful duplication of an elementary species by means of crossing, see *Oenothera biennis* × *O. cruciata* Nutt. in *Gruppenweise Artbildung*, p. 311.

expected that their hybrids, or at least some of them, may combine the different lines of mutability of their parents; and at all events, the mutability of such a hybrid would be no proof that this phenomenon may be produced by means of crossing. On the other hand, if the species to be crossed, or even only one of them, were not pure, the hybrid might inherit this impurity and show phenomena which might easily be mistaken for mutations.

It so happens that *O. biennis* is in a condition of mutability analogous to that of *O. Lamarckiana,* although not developed to the same high degree. From time to time it produces dwarfs, which are distinguished from it by exactly the same two characters which differentiate the dwarfs of *O. Lamarckiana* from their mother species, namely, low stature and sensitiveness to the attacks of some species of soil bacteria.[2] Moreover, Stomps has shown that *O. biennis* may, although very rarely, double the number of chromosomes in its sexual cells, which in *O. Lamarckiana* produces the two mutants *O. gigas* and *O. semigigas.*[3] As is now generally admitted, *O. gigas* results from the pairing of two mutated sexual cells, each of which has a double number of chromosomes. *O. semigigas,* on the other hand, is produced by the pairing of a sexual cell mutated in the same way, with a normal gamete; therefore it possesses only 21 chromosomes (14+7), while the number in *O. gigas* is 28. As yet, only *semigigas* mutants have been observed coming from *O. biennis,* and it is obvious that the double combination must be much rarer. As a proof of this special kind of mutability in *O. biennis,* however, the observations of Stomps are wholly sufficient.

In quoting these facts, Davis says that if it can be shown "that tested strains of the *biennis* are able to produce new forms of specific rank or even marked varieties, the mutationists would have much stronger evidence in support of the mutation theory than that based on the behavior of *O. Lamarckiana.*"[4] After conceding this strong position to his adversaries, Davis subjects the results of Stomps to a rather sharp criticism, which, unfortunately, is based upon a confusion of two wholly distinct types, namely, *O. biennis*

[2] Stomps, Th. J., "Mutation von *Oenothera biennis* L.," *Biol. Centralbl.* XXXII, 1912, 521-535; also Zeylstra, H. H., *"Oenothera nanella* De Vries, eine krankhafte Pflanzenart," *Biol. Centralbl.* XXXI, 1911, 129-138. Cf. also: *Gruppenweise Artbildung,* 1913, 296-304.

[3] Stomps, Th. J., *op. cit.,* p. 533.

[4] Davis, B. M., "Mutations in *Oenothera biennis* L.?" *Amer. Nat.,* XLVII, 1913, 116-121 (especially p. 116); see also *loc. cit.,* 540-596 (especially p. 567).

L. var. *cruciata*[5] and *O. cruciata* Nutt. He says: "It should be made clear that the form (*O. biennis cruciata*) is recognized in the more recent taxonomic treatments as a true species sharply distinguished from types of *biennis* by its floral characters," and "a cross between these types must certainly be regarded as a cross between two very distinct evolutionary lines and its product as a hybrid in which marked modifications of germinal constitution are to be expected."[6] But, as a matter of fact, the Dutch *O. biennis cruciata* differs from *O. biennis* only in the characters of the petals; in all other respects it is wholly the same, and therefore evidently only a subordinate variety of this species. It has not been dealt with in recent taxonomic treatments, since it occurs almost exclusively in the sand dunes of Holland, where it is produced from time to time by mutation from the mother form (first observed in 1900), without having been able until recently to multiply in the field so as to produce a persistent local variety.[7]

On the other hand, *O. cruciata* Nutt. is quite a different species, with narrow, brownish green leaves, and a different type of branching, of spikes, and of fruits. It grows wild in New York and Vermont, and is well known to all students of the American flora. By some authors it has been considered a variety of *O. biennis*, and this probably is the chief cause of Davis's confusion. The character and the behavior of its hybrids with *O. biennis* have been amply dealt with in my *Gruppenweise Artbildung.*

In the experiment of Stomps, the dwarf and *semigigas* mutations were produced by hybrid strains of *O. biennis* and *O. biennis cruciata,* and it was assumed that such strains would behave as true species in all characters not related to the differentiating marks of the petals. It must be conceded, therefore, that the cross of these two forms may be treated "as though it were the combination of forms within the same species, which have similar germinal constitutions" (Davis, *op. cit.*, p. 117).

But the most clear and simple way of obviating this whole objection is evidently to sow seeds of *O. biennis* of pure descent upon the same large scale as in the former experiment. This has been done, and a dwarf and a *semigigas* form have been produced by this pure line, besides some other mutations.[8] They had the

[5] *Die Mutations-Theorie*, II, 599.
[6] *Amer. Nat.*, XLVII, 1913, 117.
[7] *Die Mutations-Theorie*, II, 599.
[8] Stomps, Th. J., "Parallele Mutationen bei den Oenotheren." *Ber. der Deutsch. Bot. Gesell.*, XXX, 1914, No. 3.

same characters as the former one, and now provide us with the "strong support" asked for by Davis. Moreover, they show that his choice of *O. biennis* for a proof of the assertion that mutability might be produced by crossing immutable species was a most unhappy one.

The second condition for success in this kind of work is, as has been stated, the purity of the types to be crossed. As already quoted, Davis assumes that a cross between two very distinct evolutionary lines may give a hybrid with marked modifications of germinal constitution. This may be applied to his choice of the type which he calls *O. grandiflora,* and which he has made the other parent of his initial cross. He got his seeds from Dixie Landing, Alabama, a locality where Bartram had discovered *O. grandiflora* about a century ago. He assumed them to be of the pure species, but a culture which I made in my garden from seeds kindly supplied to me by Mr. Davis proved to be a mixture, and thereby threw a distinct doubt upon the purity of the station. For this reason I visited Dixie Landing in September 1912, and had the good fortune to be accompanied by Mr. H. H. Bartlett, of Washington, well known for his systematic researches among the wild species of this group. We found the station in a most desolate condition. A small-flowered species *O. Tracyi,* in almost all respects different from *O. grandiflora* had migrated into the same old cotton fields and mixed everywhere with the species of Bartram.[9] On no single field was the original form pure; it was always mixed to such a degree with *O. Tracyi* and with their hybrids that we found it impossible to collect undoubtedly pure *grandiflora* seed from this locality. Moreover, the intermediate types were so numerous (over a dozen) that it was difficult to regard all of them as normal hybrids between only two parents. To produce such a diversity of forms, either one or both of the parents must have been in a mutating condition, or more than two species must have combined in the crosses. In both cases, the material can hardly be considered as a fit starting-point for experiments bearing upon the causal relations of crossing and mutability.

Recently I have shown that besides *O. biennis* some other species of *Oenothera* are actually in a state of mutability, and especially has one of the most common American types thrown off

[9] De Vries, Hugo, and Bartlett, H. H., "The Evening Primroses of Dixie Landing, Alabama." *Science,* N. S., XXXV, 1912, 599-601.

marked mutants in my experiment garden.[10] The degrees of development of this condition, however, are very different in different species. In some of them mutations occur rarely, but they serve to throw a doubt upon the stability of those forms for which no positive results have as yet been won. In other words, we may say that almost all the nearest allies of *O. Lamarckiana* are open to the suspicion of sharing at least some degree of the mutability of this species. There is no use, therefore, in trying to produce mutability by crosses of species of the same subgenus (Onagra) in order to show that this phenomenon is only a result of crossing, as is asserted by Davis.

Moreover, I might point out that the question should be dealt with from a general standpoint and not be limited to the evening primroses. If it should be true that phenomena like those of *O. Lamarckiana* could be produced by crossing immutable species, it would, of course, be of much higher scientific value to produce them in other families or genera, or at least in the other subgenera of the evening primroses. The chance of finding immutable parents for a cross would be far greater and the proof could be given as easily and in many cases with less amount of mechanical work and space in the garden. The line of work chosen by Davis seems to me to be necessarily without any chance of success.

Besides his experimental work, Davis has made some historical researches to discover the origin of *O. Lamarckiana*.[11] Unfortunately he has neglected to visit the Museum d'Histoire Naturelle at Paris, where the herbarium of Lamarck is preserved, and where other valuable documents concerning the first appearance of our species are to be found. For myself I visited these collections in 1895 and reported on the result of my investigations in my *Mutation Theory* (Vol. I, pp. 437-444 of the English edition). In October 1913 I repeated my visit and compared the authentic specimens with the remarks made upon them by Davis. I regret to say that, through his ignorance of the available evidence, Davis has been led to conclusions which are fully contradicted by the herbarium material, both of the "Herbier de Lamarck" and of the "Herbier général" of the Museum. As we shall see, the origin of *O. Lamarckiana* is the same as I have pointed out in my book.

In the herbarium of Lamarck, *O. grandiflora* (Lam.), which

[10] *Gruppenweise Artbildung*, pp. 296-312.

[11] Davis, B. M., "Was Lamarck's Evening Primrose (*Oenothera Lamarckiana* Seringe) a form of *Oenothera grandiflora* Solander?" *Bull. Torr. Bot.*

later was renamed by Seringe and called *O. Lamarckiana,* the name it still bears, is represented by two large flowering specimens. When I studied them in 1895, they were loose on their sheets and bore together the no. 12, indicating that they corresponded with no. 12 *O. grandiflora* of the *Encyclopédie méthodique, Botanique,* by Lamarck.[12] About 1900 they were fastened on new sheets and the numbers have been lost.[13] For convenience, I shall call these specimens *A* and *B,* the former being represented by our pl. XVII, while a photograph of *B* has been published by Davis.[14]

Unfortunately, these two specimens do not belong to the same elementary species, but the question as to which of them is to be considered as the authentic specimen has been answered by all authors in the same way, with the exception of Davis. According to the general agreement, *A* (pl. XVII) is the type of the species. Davis has not seen this specimen, and has based his judgment upon the communications of botanists concerned with systematic rather than with elementary species.

The plant *A* corresponds exactly with *O. Lamarckiana* Ser. as it is now universally cultivated and as I know it from my own cultures. The specimen is evidently a side branch, picked in the autumn, and the flowers, although very large, are not quite so large as may be seen in July and August. It bears no fruits, but the sexual organs of the flowers and the form of the flower buds do not leave the least doubt concerning its identity. The stigma lobes are widely spread and raised by the long style high above the tops of the anthers, and this is one of the best characters of *O. Lamarckiana.* The buds are conical and thick, and not thin as in *O. grandiflora* Ait. For comparison, I have given a group of flower buds (*Bot. Gaz.,* pl. XVII), picked in the autumn also, from my pure cultures. All the other marks correspond to those of the present species, although of course not all of them distinguish it from allied forms.

This sheet bears the label, "d'Amérique sept., tige rameuse,

Club, XXXIX, 1912, 519-533. pl. 37-39; "A Much Desired *Oenothera,*" *Plant World,* XVI, 1913, 145-153; "The Problem of the Origin of *Oenothera Lamarckiana,*" *New Phytol.* 233-241, 1913.

[13] *The Mutation Theory,* I, 442.

[13] The herbarium of Lamarck was acquired by the Museum d'Histoire Naturelle in 1886. Comp. Ed. Bonnet, "L'herbier de Lamarck, son histoire, ses vicissitudes, son état actuel." *Jour. Botanique,* XVI, 1902, 129-138.

[14] Davis, B. M., "Was Lamarck's Evening Primrose (*Oenothera Lamarckiana* Seringe) a form of *Oenothera grandiflora* Solander?" *Butt. Torr. Bot. Club,* XXXIX, 1912, 519-533. See pl. 37. For *A* see *Bot. Gaz.,* April, 1914.

haute de 3 à 4 pieds," in the handwriting of Lamarck. The description in the *Encyclopédie* says of the origin of the species: "Cette espèce est originaire de l'Amérique septentrionale. On la cultive au jardin du Muséum d'Histoire Naturelle (V. S.)."[15] The description, however, quotes some points which are not visible on the herbarium specimen, nor on specimen *B*. It is therefore clear that the author knew his plants from another source still, probably from the living material of the Jardin des Plantes. The most interesting point for us is the description of the fruits: "Le fruit est une capsule courte, cylindrique, glabre, tronquée légèrement, quadrangulaire, n'ayant environ que le tiers de la longueur du tube calicinal."[16] This description wholly agrees with the fruits of the present species, especially if we remember that Lamarck based his description on a comparison with the only other large-flowered form he knew, *O. longiflora*. The short fruits at once distinguish our species from the allied types, such as *O. suaveolens* Desf. and *O. grandiflora* Ait., which have thin and proportionally long fruits.[17]

This character of the fruits shows that the description of the *Encyclopédie* has been based upon specimen *A* and not upon the other one. For although *B* lacks fruits also, it belongs to an elementary species which has long and narrow fruits, as we shall soon see. Here I might point out that in systematic researches of this kind, more value is to be attached to published diagnoses and descriptions than to the material preserved in a herbarium. The older systematists, as a rule, did not take much care of their material, even if they were very careful of their descriptions.[18] The herbarium specimens are often found without their names and without any indication concerning their origin. The rule *descriptio praestat herbario* applies in our special case, even as it does in

[15] V. S. (*vidi siccum*) means that the diagnosis is based on herbarium material.

[16] *Encyclopédie méthodique, Botanique par Lamarck*, Vol. IV, 1796, pp. 550-554. "Onagraire." Twelve species of this genus are enumerated, *O. longiflora* being no. 4, *O. corymbosa* no. 11, and *O. grandiflora* no. 12. A copy of the diagnosis of this last one may be found in my *Mutation Theory* (p. 441) and in the article of Davis. The article in the *Encyclopédie* is not signed and was probably written by Poiret, who prepared many articles in Vol. IV, and wrotes the whole of the later volumes. In the herbarium of Paris some of the specimens may be seen quoted with the authority of Poiret, as, for example, on the sheet *O. suaveolens* Desf., where above that name is written "*Oenothera grandiflora* Poiret Encyclopédie." (Cf. pl. 30 of the article of Davis.)

[17] "L'*Oenothera grandiflora* de l'herbier de Lamarck," *Rev. gén. botanique*, XXV, 1914.

[18] Cf. Bonnett, *op. cit.*, p. 138.

many others. In our case, the description is relatively complete and clear, while in the dried specimen only part of the characters are represented.

For all these reasons I cannot agree with Davis, who says (p. 519) that I made an incorrect determination of the material of my cultures, when I identified it with Lamarck's plant of 1796. The authentic specimen of Lamarck and the description in the *Encyclopédie* correspond as closely with the characters of my plants as dried specimens and descriptions expressed in words ever can.

On the contrary, the specimen *B* is surrounded with doubts. Davis has given a very elaborate description of this branch, comparing it with my *Lamarckiana*. The sheet bears the label "*Oenothera* . . . . [*grandiflora*] . . . . nova spec. flores magni lutei, odore grato, caulis 3 pedalis." The fact that the name *grandiflora* is placed in brackets shows that Lamarck did not wholly trust his identification of this plant with the other one. Perhaps the words *nova species* indicate that he took it to be possibly a different species. Later, Poiret discovered the identity of this specimen with *O. grandiflora* Aiton Hort. Kew. s. p. 2,[19] as has been indicated by Davis. And in De Candolle's *Prodromus* (III, 47) Seringe separated the two types, describing *O. grandiflora* Ait. and *O. Lamarckiana* as different species.

The words *odore grato* point to *O. grandiflora* Ait., which has fragrant flowers, while the flowers of *O. Lamarckiana* Ser. are almost without odor. In the original description no mention is made of the odor, and this shows once more that the specimen *B* was not the authentic one for this description.

Davis has compared the branch *B* with some of his hybrid strains from Dixie Landing[20] and finds a close resemblance. Perhaps the plant of Lamarck was a chance hybrid found in the Jardin des Plantes, and in this case, as Davis says, "we can have no certainty as to the characters of an individual plant unless its seeds have been grown in large cultures."[21] At all events it is not backed by other herbarium material in the Museum d'Histoire Naturelle, so far as I know. If Poiret's opinion that it belongs to *O. grandiflora* Ait is correct, then it has evidently not served as a basis for the description of *O. grandiflora* Lam. (*O. Lamarckiana* Ser.). In

[19] *Encyclopédie méthodique.* Suppl. IV, p. 141. See Davis, p. 522.

[20] At Dixie Landing, Alabama, only hybrid strains of *O. grandiflora* and *O. Tracyi*, perhaps mixed with other species too, are to be found. See *Science, loc. cit.*, p. 399.

[21] Davis, B. M., "A Much Desired Oenothera," *Plant World*, XVI, 1913.

O. grandiflora the fruits are thin and relatively large, for example, 3 cm. long and 3 mm. wide; while those of *O. Lamarckiana* may measure 2.5 cm. in length and 6 mm. in width, making a ratio of $^{1}0/_{1}$ in the one case and $^{4}/_{1}$ in the other.[22] The description of the fruits as short, as given by Lamarck, evidently points to the second and not to the first case.[23]

Summing up the main results of this discussion, we find that specimen *A* of the herbarium of Lamarck closely corresponds to the *O. Lamarckiana* Ser. of the present time, and has been taken by almost all authors for its prototype. The specimen *B* differs from it in its general aspect, in the words *odore grato* on its label, and in the opinion of Poiret that it belongs to *O. grandiflora* Ait., this opinion pointing to long and narrow fruits. Personally, it impressed me as having been brought into the herbarium of Lamarck only later on, and as having been placed in the cover of *O. grandiflora* Lam. with a doubt shown by the placing of the name in brackets.

The best proof for the fact that *A* and not *B* is the authentic specimen of *O. grandiflora* Lam. is perhaps given by the specimen in the herbarium of Father Pourret, which was given to the Muséum d'Histoire Naturelle by Dr. Barbier in 1847.[24] It bears the name *Oenothera grandiflora* Lam. written in the clear and beautiful handwriting of the clerk of Pourret. In the same cover there is another sheet of Pourret's collection, on which the same clerk wrote *Oenothera biennis.* Unfortunately, Davis, who did not visit the museum, has mistaken this one for the one studied by me,[25] and has accordingly published a photograph (pl. 38) and a description of it. It is easily seen that this specimen really comes nearer to our present *O. biennis* L. than to anything else.

The plant which Pourret called *O. grandiflora* Lam. agrees wholly with the present *O. Lamarckiana* Ser., and in all respects. It was fastened on its sheet by the clerk of Pourret and consists

[22] L'*Oenothera grandiflora* de l'herbier de Lamarck," *op. cit.*, fig. 1, *b* and *c*.

[23] Davis (*op. cit.*, p. 523) lays great stress on the tips of the sepals, but I cannot find a well defined difference between the two species in this character. He calls attention to the word *sétacé* in Lamarck's description of the sepal tips: "This has been translated by De Vries (*Mutations-Theorie*, p. 317) as *dicke*. The French, however, is from the late Latin word *setaceus*, derived from *seta*, a stiff hair or bristle. The meaning, therefore, is exactly the opposite of that given by De Vries." If the reader will kindly look up my book at the page quoted by Davis, he will find that I have translated *sétacé* by *faden*förmig.

[24] *The Mutation Theory*, Engl. ed., I, 442, n. 2.

[25] *Bull. Torr. Bot. Club. loc. cit.*, p. 527.

of two flowering spikes and two separate flowers. The stigma lobes are seen spread above the anthers in the normal way. The specimens were picked at the beginning of the flowering period and bear no fruits; obviously they were main spikes. They will be recognized at once as *O. Lamarckiana* by any one who has seen living cultures of this species. As I have quoted in my *Mutation Theory* (*loc. cit.*), Spach has written on this sheet "*Onagra vulgaris grandiflora* Spach," which remark also proves the identity with *O. Lamarckiana* Ser. The printed label says "Collection de l'Abbé Pourret, extraite de l'herbier legué par M. le Dr. Barbier en 1847." The main spike measures about 40 cm., the smaller one about 20 cm.

In my book I have also referred to a specimen of *O. suaveolens* Desf. At that time I did not know the Alabama species and believed that *O. suaveolens* Desf. and *O. grandiflora* Ait. were synonyms, as almost all authors did. Therefore I used the two names promiscuously. Last summer, however, I cultivated, side by side, *O. suaveolens* Desf. from Fontainebleau, collected by Dr. Blaringhem, and *O. grandiflora* Ait. from Castleberry, Alabama, collected by myself with Mr. Bartlett. They proved to be wholly different species.[26] So far as I know, the large-flowered Oenotheras, which are now relatively common in the western departments of France, all belong to *O. suaveolens Desf.*, at least all the specimens and cultures on which I based my opinion in 1901 did. The specimen of the Muséum d'Histoire Naturelle, which I referred to especially, has been described by Davis from a photograph which is reproduced on pl. 39 of his paper. Davis, who did not know the *O. suaveolens* as a separate species, called it the flotsam of the herbarium (p. 529); it is, on the contrary, the authentic specimen of Desfontaines, bearing on the label the name *suaveolens* written by Desfontaines himself. The smaller plant, fastened on the same sheet, has another label, saying only *O. grandiflora,* and seems to me to have been fastened on this sheet subsequently. The larger one, however, corresponds exactly with the species which is now growing in many thousands of specimens near Samois on the eastern limit of the Forêt de Fontainebleau, where I visited the different stations with Dr. Blaringhem in October 1913. The long fruits and the thick flower buds do not leave the least doubt concerning the identity of this specimen.

The most interesting discovery in this field of historical re-

[26] "L'*Oenothera grandiflora* de l'herbier de Lamarck," *loc. cit.*

search, however, is that of a specimen of *O. Lamarckiana* Ser. in the collection of Michaux, described recently by Blaringhem.[27] I had the advantage of studying this sheet myself, when I visited Paris in October 1913. The printed label says "Herb. Mus. Paris, Herbier de l'Amérique septentrionale d'André Michaux." There is no further indication of the locality and no name. The specimen is a main spike, picked in the beginning of the flowering period, and without fruits. It is excellently preserved and corresponds in all respects to my cultures of *O. Lamarckiana* Ser. The lobes of the stigma are seen to be widely spread above the anthers. The flowers and flower buds are exactly those of the present species.

André Michaux died in 1802, after having traveled during twelve years through the eastern United States from the Hudson River to Carolina. His celebrated collection constitutes one of the best sources of our knowledge of the flora of those parts of America at the end of the eighteenth century, that is, of the same period in which Lamarck published his volumes of the *Encyclopédie.* His herbarium is at present at the Muséum d'Histoire Naturelle at Paris, and his plants were discribed after his death by his son François André Michaux in a book entitled *Andraeas Michaux, Flora boreali-americana, sistens characteres plantarum quas in America septentrionali collegit Andraeas Michaux.*[28] Michaux had the habit of collecting seeds of as many species as possible, besides his herbarium specimens, and of sending them to Europe to be sown.

This beautiful specimen proves that *O. Lamarckiana* Ser. was a component of the flora of the eastern part of North America at the end of the eighteenth century, and that it has come down to us as completely unaltered as may be shown by old herbarium specimens. Moreover, it tends to make it at least very probable that the European strains, or at least some of them, are derived from the importation of seeds by Michaux. The specimen *A* in the herbarium of Lamarck, designated as "d'Amérique sept." probably belonged to this same strain.

The exact situation of the locality where Michaux collected this specimen is, of course, unknown. Much stress is laid by many authors upon the fact that no wild station for *O. Lamarckiana* has

[27] Blaringhem, L., "L'*Oenothera Lamarckiana* Seringe et les Oenothères de Fontainebleau," *Rev. gén. botanique,* XXV, 1914.

[28] *Editio nova,* 1820, Paris The genus *Oenothera* is dealt with in Vol. I on p. 214; the plant is given under the name of *O. biennis.* For the ground covered by his travels, see the preface and the article of Blaringhem.

been discovered lately in any part of the United States. This argument evidently loses the main part of its weight when we know that it was observed by such a well-known botanist as Michaux. Moreover, this situation is not peculiar to *O. Lamarckiana*; on the contrary, the same condition prevails for the other European species, *O. biennis* L., *O. muricata* L., and *O. suaveolens* Desf., whose original stations in the United States and Canada have not been rediscovered. Even *O. grandiflora,* which is known to occur in Alabama in different localities, is observed there to grow on cultivated soil only, especially on old fields of corn and cotton, and no one knows whence it came. Therefore, if our present ignorance of the origin of *O. Lamarckiana* is adduced in order to throw a doubt on its reality as a good species, the same doubt is attached to its nearest allies, and in fact, to all the dozens of elementary species of the group Onagra which are now being found wild on waste fields and along roadsides all through the United States. Autochthonous stations are not known for any of them.

A most valuable contribution to the clearance of the historical data concerning the origin of *O. Lamarckiana* Ser. has been brought forward by Davis in his criticism of the alleged Texan origin of the present cultivated strain. This was introduced into the trade by Messrs. Carter and Co. of High Holborn in the neighborhood of London, about the middle of the last century. These horticulturists offered the seeds as coming from Texas. But, since then, no botanist is known to have seen the plant in that state, and Davis suggests (p. 523) that the statement might, perhaps, have been caused by a mistake.[29] Now, it is well known that such details are, as a rule, given more in the interest of advertising than in that of pure science. Moreover, no horticulturist likes to offer for sale seeds with the announcement that the same form may be found as a wild flower in his own country.

O. Lamarckiana has been, for many years at least, a component part of the flora of England, growing in many localities, especially on the sand dunes along the coast. The most universally known station is that of St. Anne's on the Sea, near Liverpool, which has been studied by Bailey, Gates, and other botanists, and where the species occur in thousands of specimens. Davis received seeds from different English stations and recognized the plant in the cultures derived from them (*op. cit.*, p. 237). In Lancashire the species locally grows together with *O. biennis* L., exactly as it does

[29] See Davis in *New Phytol.*, XII, 1913, 234.

in the sand dunes of Holland. In such cases it produces hybrids such as I have described under the names of *laeta* and *velutina*, and as Davis has isolated as small-flowered races from those English localities (p. 237).

Now, if we agree with Davis that the seeds of Carter and Co. were derived from some English station, the probability at once arises that these English stations themselves owe their origin to the introduction of seeds from America, either by Michaux himself or by some other botanist of the same period. The history of the species would then become a very simple and clear one. In this respect it becomes of interest to look at the figure published in 1807 in Smith's *English Botany* (Vol. VI, pl. 1534).[30] According to the description accompanying this plate, the "specimen was gathered on the extensive and dreary sand banks on the coast a few miles north of Liverpool, where millions of the same species have been observed by Dr. Bostock and Mr. John Shepherd growing perfectly wild and covering large tracts between the first and second range of sand hills." In this same locality *O. biennis* L. and *O. Lamarckiana* are now growing in the same abundance of individuals, partly separated and pure in different valleys and partly in mixtures which are known to contain also their hybrids. The specimen of 1807 is designated *O. biennis*, but both the flowers have the lobes of their stigma above the anthers, which is a differentiating mark of *O. Lamarckiana*. Moreover, it is the only decisive details, all other characters of the figures applying equally to both species. If it is allowable to trust to this detail, we should be entitled to conclude that the station of Liverpool contained both forms as early as 1807, even as it is known to do at the present time. In this case, *O. Lamarckiana* must be assumed to have been introduced into England about the time of Michaux and Lamarck, and a common origin for the specimens of their herbaria and the wild stations in England becomes highly probable.

The strain of Carter and Co. has been identified by *Lindley* as *O. Lamarckiana* Ser., and the high authority of this eminent botanist confirms my own determination of the same strain, made by comparing it with the authentic specimen of Lamarck.[31]

At all events, the adduced facts indicate a very simple history of our species, which has come down to us unchanged, so far as

[30] Cf. Davis, *op. cit.*, p. 532.

[31] Davis says (*op. cit.*, p. 531): "The identification by Lindley of these plants with *O. Lamarckiana* Ser. was undoubtedly incorrect," but he does not give any reason for this assertion.

we know, from the original American habitat.[32] There is no reason to suppose that it originated as a garden plant, and none at all to subject it to all the doubts ordinarily brought forward against the purity of descent of horticultural forms in general, simply on the ground that some garden plants are of known hybrid origin. *O. Lamarckiana* has remained unchanged through more than a century, and has kept as true to its type as any good wild species. "It is exceedingly fortunate," says Davis (*op. cit.*, p. 527), "that the plant which serves as the type of *Oenothera Lamarckiana* Ser. should have come down to us so well preserved that there is scarcely a doubt of its identity." But the identity is with the species as it is still known under that name. Whether the species was in the same condition of mutability at the time of its first appearance, as it is now, is, of course, a different question.[33]

Summing up the results of this historical investigation, we may say:

1. *Oenothera Lamarckiana* Ser. is represented by specimens in the herbaria of Lamarck, Pourret, and Michaux, and is, so far as this material enables us to judge, at the present time exactly the same plant as it was at that period. It has come down to us, through more than a century, as unaltered and as constant as true species usually do.

2. It has been a component of the flora of the eastern United States, where Michaux collected it and whence Lamarck derived his specimen.

3. At the present time it is a component of the flora of England, and is as well established in that country as is *O. biennis* in different parts of Europe.

4. The strain which is now in cultivation, and which was introduced into the trade about the middle of the last century, was probably derived from some wild English locality, which itself may have come from an introduction into Europe of the seeds, collected either by Michaux himself or by some other botanist of his period.

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[32] Davis says (*op. cit.*, p. 530) "that *Lamarckiana* has come down to us greatly modified, that its parentage is far from pure, that it is in fact of hybrid origin." This assertion, which is not based upon any facts, is clearly contradicted by the preservation in excellent condition of the three specimens of Lamarck, Pourret and Michaux, not known to Davis.

[33] "Ueber die Dauer der Mutationsperiode bei *Oenothera Lamarckiana*," *Ber. Deutsch. Bot. Gesells.*, XXIII, 1905, 382.

UMI

PANDIAGONAL PRIME NUMBER MAGIC SQUARES.[1]

For ordinary magic squares the only requirements are that all rows, all columns, and the two main diagonals sum to the constant. For pandiagonal magics it is required in addition that *all* diagonals also sum to the constant.

PANDIAGONAL MAGIC SQUARES OF EVEN ORDERS.

Dr. C. Planck has given us the following rule for transposing all associated magic squares of even orders into pandiagonal magics but the inverse change from pandiagonal to association does not follow in every case.[2]

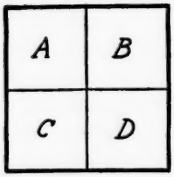

Fig. 1.

73	41	13	113
23	103	83	31
107	7	47	79
37	89	97	17

Fig. 2.

197	71	83	163	37	79
17	97	109	199	179	29
167	151	53	19	103	137
47	173	131	13	139	127
11	31	181	193	113	101
191	107	73	43	59	157

Fig. 3.

271	1	227	353	101	163	503	421
491	439	151	337	137	61	311	113
269	383	47	197	457	443	11	233
131	23	479	281	467	179	79	401
409	347	7	89	239	503	283	157
373	449	199	397	19	71	359	173
53	67	499	277	241	127	463	313
43	331	431	109	379	487	31	229

Fig. 4.

[1] The author is indebted to Messrs. W. S. Andrews and H. A. Sayles, both of Schenectady, N. Y.: to the former for valuable assistance in writing these papers and to the latter for execution of the diagrams.

[2] See *The Monist* for July, 1910, Vol. XX, No. 3, "The Method of Complementary Differences," by W. S. Andrews.

Rule: Divide the square into equal quarters parallel to its sides (see Fig. 1).

Leave A untouched.

Reflect B.

Invert C.

Reflect and invert D.

By this rule the associated magic squares of the fourth, sixth, eighth and tenth orders, (Figs. 2, 3, 4, 5), were transposed from the associated squares shown in the preceding article.

953	607	113	349	919	347	827	337	181	317
127	257	751	421	727	131	23	907	947	659
167	757	571	929	137	547	761	7	103	971
397	109	359	179	739	709	373	977	719	989
557	307	541	911	491	467	53	107	839	677
643	163	653	809	673	37	383	877	641	71
859	967	83	43	331	863	733	239	569	263
443	229	983	887	19	823	233	419	61	853
281	617	13	271	601	593	881	631	811	251
523	937	883	151	313	433	683	449	79	499

Fig. 5.

PANDIAGONAL MAGIC SQUARES OF ODD ORDERS.

It is obviously impossible to construct a pandiagonal magic square of the third order since all squares of this order are made from the same formula which is not a pandiagonal one.

x	y	z	v	t
v	t	x	y	z
y	z	v	t	x
t	x	y	z	v
z	v	t	x	y

Fig. 6.

a	b	c	d	e
c	d	e	a	b
e	a	b	c	d
b	c	d	e	a
d	e	a	b	c

Fig. 7.

1	19	53	101	163
71	157	67	13	29
79	23	47	127	61
103	31	73	89	41
83	107	97	7	43

Fig. 8.

Pandiagonal magics of odd prime orders larger than the third can be made from La Hirian primaries constructed as shown in Figs. 6 and 7, the paths in this instance being knight's moves in different directions.

A pandiagonal magic square of the fifth order is shown in Fig. 8 in which

$x = 1$	$a = 0$
$y = 13$	$b = 6$
$z = 23$	$c = 30$
$v = 41$	$d = 60$
$t = 97$	$e = 66$

and the summation = 337.

1	19	53	101	163	751	857
601	653	211	283	23	47	127
311	97	547	617	61	79	233
43	83	107	307	811	587	7
797	271	13	29	71	157	607
103	571	647	67	223	293	41
89	251	367	541	593	31	73

Fig. 9.

Fig. 9 shows a pandiagonal 7^2, S = 1945, the primaries being as follows:

$x = 1$	$a = 0$
$y = 13$	$b = 6$
$z = 23$	$c = 30$
$v = 41$	$d = 60$
$t = 97$	$e = 66$
$s = 541$	$f = 210$
$p = 587$	$g = 270$

After long perseverance the writer succeeded in finding the unbalanced series of primes for a magic square of the 9th order,

shown in Fig. 10, but there appears to be no rule for making a pandiagonal magic therefrom. It was therefore decided to approach pandiagonal or nasik results as nearly as possible.

	1	7	11	137	181	251	631	827	2647
	13	19	23	149	193	263	643	839	2659
	31	37	41	167	211	281	661	857	2677
	61	67	71	197	241	311	691	887	2707
	103	109	113	239	283	353	733	929	2749
	433	439	443	569	613	683	1063	1259	3079
	1483	1489	1493	1619	1663	1733	2113	2309	4129
	2341	2347	2351	2477	2521	2591	2971	3167	4987
	5647	5653	5657	5783	5827	5897	6277	6473	8293

Fig. 10.

826	6	180	2646	10	250	630	0	136
630	0	136	826	6	180	2646	10	250
2646	10	250	630	0	136	826	6	180
6	180	826	10	250	2646	0	136	630
0	136	630	6	180	826	10	250	2646
10	250	2646	0	136	630	6	180	826
180	826	6	250	2646	10	136	630	0
136	630	0	180	826	6	250	2646	10
250	2646	10	136	630	0	180	826	6

Fig. 11.

1	13	31	61	103	433	1483	2341	5647
0	6	10	136	180	250	630	826	2646

The above La Hirian primaries being laid out in the usual way from the unbalanced series, two primary squares (Figs. 11

and 12) were made according to an arrangement formulated by Mr. H. A. Sayles, Schenectady, N. Y. A combination of these two primary squares produced the magic square shown in Fig. 13,

433	31	5647	61	1	1483	103	13	2341
61	1	1483	103	13	2341	433	31	5647
103	13	2341	433	31	5647	61	1	1483
5647	433	31	1483	61	1	2341	103	13
1483	61	1	2341	103	13	5647	433	31
2341	103	13	5647	433	31	1483	61	1
31	5647	433	1	1483	61	13	2341	103
1	1483	61	13	2341	103	31	5647	433
13	2341	103	31	5647	433	1	1483	61

Fig. 12.

S = 14,979. In this square each third diagonal = S, so that only six diagonals are regular. Each pair of irregular diagonals shows summations $s + x$ and $s - x$; x being different with each

1259	37	5027	2707	11	1733	793	13	2477
691	1	1619	929	19	2521	3079	41	5897
2749	23	2591	1063	31	5783	887	7	1669
5653	613	857	1493	311	2647	2341	239	643
1483	197	631	2347	283	839	5657	683	2677
2351	353	2659	5647	569	661	1489	241	827
211	6473	439	251	4129	71	149	2971	103
137	2113	61	193	3167	109	281	8293	443
263	4387	113	167	6277	433	181	2309	67

Fig. 13.

pair in this square. The writer has attempted to correct these faulty diagonals, but so far without success.

In order to build a nasik square of the 9th order by rule, it is

necessary to find a series which will permit the two sets of La Hirian primaries to be each divided into three groups of numbers having a common summation, as exemplified in the arithmetical series 1, 2, 3, 4.....81. In this series the two sets of La Hirian primaries are

1	2	3	4	5	6	7	8	9
0	9	18	27	36	45	54	63	72

and the above may be rearranged in three groups as follows:

1	5	9	3	4	8	2	6	7
0	36	72	18	27	63	9	45	54

in which each triplet in the upper line sums 15 and each triplet in the lower line sums 108. The difficulty of finding a 9^2 series of prime numbers that will meet the above conditions appears to be insurmountable.

The writer believes the squares in this paper to be of the lowest possible summations, but no claim to that effect is made except in the case of the 4^2, though it is probable that 5^2 and 7^2 are also of minimum summation.

It is hoped that some student of magic squares may be able before long to make a pandiagonal square of the 9th order with prime numbers.

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PANELED MAGIC SQUARES.*

These squares are made with a central magic square having one or more panels or borders of figures, so arranged that each enlargement forms another magic square.

Paneled magic squares may be either "perfect" or "imperfect," the former being those in which all intermediate squares are magic, and the latter those wherein one or more of the intermediate squares are not magic.

These two varieties are constructed by different rules. The "perfect" squares are formed entirely of couplets, with the exception of the center cells of odd squares, and the inner 4^2 of even squares,

* Diagrams by Mr. H. A. Sayles, Schenectady, N. Y.

while "imperfect" squares may be made with couplets, triplets, quadruplets, etc.

Only perfect paneled squares will be considered in this article and these will be divided into two classes wherein n is respectively odd and even. Furthermore, only prime numbers will be used in their construction.

PANELED MAGIC SQUARES IN WHICH n IS ODD.

In constructing squares of this class of all orders, it is necessary to find

$$4+8+12.....+2(n-1)$$

couplets, each summing twice a certain prime number which must occupy the center cell of the square. The prime numbers forming these couplets must also contain a 3^2 series answering to the following formula:

a	$a+b$	$a+2b$
$a+c$	$a+b+c$	$a+2b+c$
$a+2c$	$a+b+2c$	$a+2b+2c$

After the 3^2 is made from this series the remaining couplets must then be arranged in magic order.

Paneled Magic Square of the 5th Order.

It is obvious that this is the smallest possible paneled square, and it must be formed of a magic 3^2 which is part of and entirely enclosed in a magic 5^2. Twelve couplets must be used in its con-

401	11	263	149	431
419	461	23	269	83
311	59	251	443	191
53	233	479	41	449
71	491	239	353	101

Fig. 1.

struction, each couplet summing twice the prime number which is to occupy the center cell. Twelve such couplets can be found, each summing 466, but as 3 is one of the constituent numbers, it can be easily proved by analysis of the congruences (mod. 3) that it is impossible to make a magic square with these numbers. In the next higher available series, each couplet sums 502, and a paneled magic

square made with these couplets is shown in Fig. 1. In the 5^2, S = 1255 and in the 3^2, S = 753.

Paneled Magic Square of the 7th Order.

For this square a series of 24 couplets is required, and $617 \times 2 = 1234$ is the lowest number which will permit this series of couplets. This series, however, contains a 3 and is therefore inadmissible.

461	1193	311	419	1229	353	521
1151	1277	1049	173	443	263	131
1013	59	1181	23	719	1223	269
191	599	179	641	1103	683	1091
509	251	563	1259	101	1031	773
401	1019	233	1109	839	5	881
761	89	971	863	53	929	821

Fig. 2.

The next series sums $641 \times 2 = 1282$, and Fig. 2 shows a 7^2 which has been made with these couplets, the different summations being, $7^2 = 4487$, $5^2 = 3205$ and $3^2 = 1923$.

Paneled Magic Square of the 9th Order.

This square demands 40 couplets, each summing twice the prime number which must occupy the center cell. Two sets of 40

2087	2441	2477	191	887	863	857	593	1907
2267	1163	2711	1451	2423	71	1097	653	467
2381	2633	41	2729	1553	1361	1151	101	353
2351	1721	2687	2273	401	1427	47	1013	383
317	113	1223	521	1367	2213	1511	2621	2417
491	947	1301	1307	2333	461	1433	1787	2243
761	911	1583	5	1181	1373	2693	1823	1979
821	2081	23	1283	311	2663	1637	1571	1913
827	293	257	2543	1847	1871	1877	2141	647

Fig. 3.

couplets can be found summing 2554 and 2722 respectively, but as each series contains a 3, neither is admissible. There are 41 couplets

summing 2734, one of which contains a 3, and a paneled magic square constructed from the remaining 40 couplets is shown in Fig. 3.

The 3^2 sums 4101, the 5^2 6835, the 7^2 9569 and the 9^2 12303, respectively.

PANELED MAGIC SQUARES IN WHICH n IS EVEN.

To construct these squares

$$10+14+18.....+2(n-1)$$

couplets are required. These couplets need not sum twice a prime number as in squares where n is odd. The panels are made as in the odd squares and the inner 4^2 is formed from such primes as remain over. The summation of the inner 4^2 must be twice that of the couplets.

Paneled Magic Square of the 6th Order.

Ten couplets are required to construct this square. Eleven couplets of prime numbers may be selected summing 144, but a 4^2

97	43	139	1	67	103
61	31	71	7	191	89
23	13	167	101	19	127
109	251	3	29	17	41
113	5	59	163	73	37
47	107	11	149	83	53

Fig. 4.

summing 288 has not yet been found, and it is doubtful if it can be made from the series of prime numbers remaining after the outside panel is finished. The next lowest summation for a series of ten couplets is 150, and a square constructed with these couplets is shown in Fig. 4, wherein the 6^2 has S = 450 and the 4^2 has S = 300.

Paneled Magic Square of the 8th Order.

Twenty-four couplets are necessary for this square, and these can be found summing 330, but they do not appear to contain a 4^2 series. The lowest possible summation for these couplets containing

a 4^2 series is 390, and the square made with them is shown in Fig. 5, wherein the 8^2 sums 1560, the 6^2 1170, and the 4^2 780.

Panęled Magic Square of the 10th Order.

Forty-two couplets are needed for this square, and the lowest possible summation for same is found to be 660. Fig. 6 shows a

379	233	17	151	227	37	149	367
127	197	389	277	7	109	191	263
59	41	3	179	107	491	349	331
139	79	467	269	13	31	311	251
317	347	53	19	479	229	43	73
293	307	257	313	181	29	83	97
223	199	1	113	383	281	193	167
23	157	373	239	163	353	241	11

Fig. 5.

61	587	563	487	103	157	449	571	239	83
137	19	241	263	647	643	409	389	29	523
193	43	349	1	313	281	653	383	617	467
139	59	307	31	107	499	683	353	601	521
197	229	67	79	719	331	191	593	431	463
547	619	613	941	37	233	109	47	41	113
479	607	367	269	457	257	337	293	53	181
509	433	277	659	347	379	7	311	227	151
461	631	419	397	13	17	251	271	641	199
577	73	97	173	557	503	211	89	421	599

Fig. 6.

square made with these couplets, the various summations being, $10^2 = 3300$, $8^2 = 2640$, $6^2 = 1980$ and $4^2 = 1320$.

It will be seen that this brief paper covers all orders of paneled magic squares made with prime numbers up to the 10th order inclusive, and it is believed that all these squares show the lowest possible summations.

WYOMING, N. J. CHAS. D. SHULDHAM.

LATEST LIGHTS AND SHADOWS ON THE JESUS-QUESTION.

I.

In the Waterloo that has now been raging for eight and more especially for three years around the name of Jesus, the Josephine passage (*Ant.*, XVIII, 3, 3; Niese's ed., XVIII, 63, 64) has occupied the position of La Haye Sainte. Ever since the birth of criticism the section has been regarded as spurious by nearly every unfettered inquirer. Scaliger and Casaubon expressed themselves orally and timidly, for one ran considerable risk in doubting the genuineness, but Salmasius and Gronovius boldly in writing, and Blondell and Faber (about 1657) still more boldly. Dubos came to the defense, and there was a lull till Eichstädt renewed the attack in his Jena Program of 1813. Since then the battle has gone more and more against the genuineness, till in 1913 Harnack knew not of a single reputable Protestant savant that defended it, Ranke's and Von Goldschmidt's plea for its partial genuineness being regarded as merely a curiosity. Then it was that F. C. Burkitt, of Cambridge, in the *Theol. Tijdschrift* advanced boldly to the rescue of the testimony, and quickly came to his side Harnack and, later, Professor Barnes in the *Contemporary Review*, January 1914, pp. 57-68.

The reason for this recrudescence of faith in Josephus is not far to seek. In the preceding century the historicity of Jesus had not been seriously questioned, and the critics allowed the obvious facts in the case to have their due weight. Of late, however, all this has suffered a sea-change. Historical? or unhistorical? has become a "burning question of the day," and the need of some external testimony has become importunate, and the anguish at its absence has become poignant almost to paroxysm. "New and more efficient weapons will have to be forged," admits Klostermann, and what else could be half so efficient as the genuineness of the Josephus-passage, which would be the end of controversy?[1]

How has this new apology been received? With practically unanimous rejection. Of course, the multitude have shouted assent, the religious journals have expired a sigh of immense relief, but honest and self-respecting critics, like Linck and Windisch, have not

[1] Unless, as Harnack thinks, Josephus might have "read his account out of the Gospels," in which case his testimony would be "quite indifferent."

been moved in the least. Especially E. Norden, the colleague and compeer of Harnack, has gone over the whole field of controversy from the earliest to the latest, and has dissipated the contentions of Harnack absolutely and completely. Seldom has a work of destruction been so thoroughly done. Norden not unnaturally hopes that we may hear no more of this vain apology, but herein he underrates the toughness of the fibre of faith and the felt necessity of showing that the contemporary Jewish historian and gossiper did take notice of the alleged presence in his own neighborhood of the most impressive and most conspicuous personality the world has ever seen.

It is not strange then that P. Corssen in his article on "The Testimonies of Tacitus and Pseudo-Josephus concerning Christ," read January 20, 1914, in the *Religionswissenschaftliche Vereinigung* in Berlin, published in *Preuschen's Zeitschrift*, May 1914, 2, pp. 114-140, while conceding fully that the passage is a Christian forgery, nevertheless cherishes the suspicion, or rather the hope, that some phrase of Josephus concerning Christ may possibly have provoked the forgery. His reasons for thinking that the phrase "receive the true with pleasure"[2] and "the first among us"[3] are remnants of an ancient Josephine text, seem almost incredibly flimsy. Norden has shown that "receive with pleasure" occurs seven times in this eighteenth book of the *Antiquities*, so that the interpolator could hardly have failed to know that it was a favorite with his author and so would have been tempted to use it in imitation.

Equally important is the fact, which Norden should have noted, that both the phraseology and the idea are good evangelic. "Receive" is a New Testament favorite: "with joy receive the word" is in Luke viii. 13; "receive the word with all readiness," Acts xvii. 11; "the love of the truth they received not," 2 Thess. ii. 10. Harnack indeed protests that a Christian would not have used the word "pleasure"[4] since the word was commonly applied to sense-"pleasure"; but the word is certainly used at least once by a Christian preacher in the thoroughly Christian sense of the joy that comes from performing divine commands (II Clem. Rom. 15): "For however great a pleasure have these words for such as do them, so great a condemnation have they for the disobedient"; moreover, and this is the important point, the dative[5] is a mere

[2] *τῶν ἡδονῇ τὰ ἀληθῆ δεχομένων.*
[3] *τῶν πρώτων ἀνδρῶν παρ' ἡμῖν.*
[4] *ἡδονή.*
[5] *ἡδονῇ.*

adverb and quite equivalent to the adverb,[6] which is used twice in Mark (vi. 20; xii. 37), "the multitude heard him with pleasure," which exactly agrees with the Josephine expression. We too use the noun pleasure (s) generally of the senses; but when one is asked, "Will you do me a favor?" and replies, "Certainly, with pleasure," one means simply gladly, and the sense-reference has quite disappeared. This argument, then, so stressed by Harnack, has no force.

The second phrase criticised by Corssen has its parallel, as he himself admits, both at the beginning and at the end of the *Antiquities,* in the phrase, "the chief priests among us."[7] It is idle to say that the forger would have imitated some more familiar phrase of Josephus.

Corssen himself attaches little weight to such trifles, yet he "nevertheless believes" that he herewith "strengthens the probability of the general reasons that Josephus must have devoted a remark to the fate and significance of Jesus." He thinks further that Luke's professedly exact data as to the date of Jesus's appearance (iii. 1, 2) may have come from Josephus, "who belonged to the priestly circles in Jerusalem, who would have known all about it." Such a reason seems lighter than gossamer. Where, we wonder, did Luke get his equally exact data about the census (ii. 1-3)? If the latter be fictive, as now admitted, why not the former? The combinations that follow in Corssen's articles are equally arbitrary, mere exercises of the imagination. He himself feels and admits as much, for he says towards the end (p. 138), that "it may be better to set bounds to fancy and not pursue the question further." These fantasies are not in themselves plausible, and they lose all assignable probability in the presence of countless "various possibilities."

A general observation may here be allowed. In criticism nothing is more common than to confound the adequateness of an explanation with its probability, and to reason that because it is adequate, therefore it may be accepted and used in further constructions as highly probable. Far from it. An explanation, in itself likely enough, may be very unlikely in the presence of many other possible explanations, none so inherently likely. A favorite racer may look as if he would certainly win, and his record may be excellent, but his chance against the field may all the while be very

[6] ἡδέως.

[7] τῶν παρ' ἡμῖν ἀρχιερέων.

small, and he may actually lose in the race. It is perfectly natural and easy for an ace to turn up in throwing a die, but its chance is only one in six, simply because there are five other faces just as good. When Corssen imagines all sorts of things about Josephus making obscure allusions to the burning of Rome and about Poppæa's slandering the Christians to Nero, not even if his conjectures were far more ingenious, could they have any standing in court, because there is an indefinite number of other ways in which it all might have happened. The unknown historical conditions are practically infinite.

The value of Corssen's article consists not so much in these vagaries of conjecture as in the clear statement of certain facts and in various important admissions, whose full significance he has not indicated. He observes that in Tacitus the notice about "the founder of Christianity and the judgment upon his religion" sits loose in the context (p. 135). He might have added that the whole section is still more plainly parenthetical. "The notice can be removed without disturbing the connection," hence he infers that "the account is from a contemporary source, the notice Tacitus has added." By the same token the whole section has been added, for exactly the same reasons apply with still greater force. The connection is not only not disturbed, it is visibly improved by removing the chapter. Moreover, there are many other reasons set forth in *Ecce Deus,* which are certainly impressive (witness Windisch), which Corssen makes no attempt to answer. One of the minor was the use of the form *humani generis,* instead of the uniform Tacitean *generis humani.* Corssen, however, recklessly quotes this expression twice, both times in the form *odium generis humani* (which T. *would* have used), *never* in the form *odio humani generis* (which the passage actually uses)! Another example of such un-German carelessness is the translation of *heteron ti deinon* as "Something else terrible," which misses the force of *heteron,* "the second of two," and confounds it with *allon,* "something else." Only when we translate correctly do we feel the close connection of Sections 2 and 4, and how it suffers from the insertion of Section 3.

Very valuable is Corssen's declaration that "the tendency to fabricate documents in which the Christian faith was attested even in extra-Christian circles, shows itself at a very early date" (p. 129). *Will historicists all please take notice?* We now ask, would such inventive Christians have hesitated to interlard Tacitus also with an "attestation"? Would they have shrunk from inserting a sig-

nificant phrase here and there in their own documents, both Gospels and Epistles? And since they have admittedly inserted a score, why not twoscore, why not fourscore as well? Is not, in any doubtful case, *the hypothesis of interpolation* antecedently probable?

Very welcome also, and thoroughly just, is his avowal of the primitive unhistorical character of the Gospel stories. The formula "suffered under Pontius Pilate," he tells us, is relatively young. It springs from a time in which an *historic faith* had already developed, for the earlier period it would have been quite without significance (p. 126). "We see indeed how soon the interest of the Gospel tradition in the Personality increases and fancy works on the picture." "None of the evangelists intends in the story of the passion to use Pilate to determine the time of Jesus's death." "The oldest proclamation of Jesus is entirely timeless." Not till Luke (near 100 A. D.) do we enter upon "the new phase. It is the first conscious attempt of a Christian to write history, but reaches no further than the introduction." "It introduces the narrative as real history." Had he possessed sufficient knowledge and wielded such a pen as Corssen's, the author of *Ecce Deus* might have been glad to write such sentences. Enough of this fascinating essay, which lends itself so readily to transcription, but *how in its light will naive historicism adjust its glasses?*

Corssen holds the balance pretty even between his colleagues: He praises Harnack and agrees with Norden. Perhaps it may be in place now to test "the mastership of the trained dialectician." Harnack's paper has had great currency, it is time to try its ring. He argues thus:

1. The notice of Jesus in the passage about James (*Ant.* XX, 9, 1) presupposes Jesus as known; it could hardly have been written by Josephus unless he had elsewhere mentioned Jesus, and he certainly mentions Jesus nowhere else: therefore he must mention Jesus here, *Ant.*, XVIII, 3, 3.

Indeed the dialectic does seem to be excellent. Almost precisely so it is argued in *Ecce Deus*, that since the passage about James presumes an earlier mention of Jesus, and *since there is no earlier mention*, therefore the James passage is also spurious. The sword is two-edged and cuts both ways. Which is right? Since Niese, Norden, Corssen, Linck, Windisch, and nearly all others, *reluctantly* admit the Jesus-passage to be spurious, while only a very few, such as Burkitt and Harnack, force themselves to defend

it, though *all desire with deep desire* that it be genuine, if we merely take the vote, it goes overwhelming against the genuineness: but in that case what becomes of the James-passage? Can it be an insertion? *Most easily,* in spite of Norden's and others' claim that it belongs to the context. It does not. Withdraw the words *ton adelphon Iesou tou legomenou Christou-Jakōbos onoma autōi* and *kai heterous,* and the passage reads "Ananus, then, being such as I have said, fancying he now had a fitting opportunity, since Festus was dead and Albinus was still on the road, assembles a Sanhedrin of Judges, and having brought thither certain ones and having made accusations against them as law-breakers he delivered them to be stoned." Where will you find smoother reading? It is precisely the introduction of James, the Just, that reduces the passage to nonsense; for whatever else James the Just was, a *law-breaker* he was not, but the most fanatical *law-keeper,* even in the New Testament and still more without, as in Hegesippus as cited by Eusebius (H. E., II, 23, 4-18), whose account of the death of James can never be reconciled with this Josephus-interpolation. Besides, the proper appellation of James was not "brother of Jesus the so-called Christ," but "brother of the Lord," a very different thing. The phrase *Jesus the so-called Christ,* is a New Testament phrase and a late one, occurring three times, in Matt. i. 16; xxvii. 17, 22. We can hardly covet the "style-sense" that can attribute it to Josephus. Critics have felt that the James-passage inhered in the context, it seems, simply because they failed to strike out the following words *kai heterous,* which were necessitated by the insertion of the words just before. Of course, it is not meant that all the inserting must have been done by one and the same hand; there may have been two or more working at long intervals. There is then no reason for retaining the James-passage and many reasons for rejecting it, whereby Josephus is rid of all allusion to Jesus and becomes intelligible.

2. The Jesus passage impresses Harnack as insincere—but do we expect much sincerity in a forgery? Such interpolations do often betray themselves in this way.

3. Nay more, it seems to him *patronizing.* And why not? The interpolator is acting the part of Josephus as best he can. Naturally enough he is artificial, stilted, and frigid.

4. The passage says naught good about Christians, but calls them a *nation,*[8] which they never call themselves. But why should

[8] φῦλον = *Sippe.*

the interpolator represent Josephus as saying good of Christians? Why needlessly betray himself? Can we credit him with no worldly wisdom? As to the term *Phylon*, it seems not far away from *Phyle*, the regular term for tribes of Israel, but is rather fitter in place in this passage. In the *Octavius* (VIII) of Minucius Felix the Christians are called exactly similarly a "light-fleeing nation" (*latebrosa et lucifuga natio*). But, says Harnack, that was a heathen speaking. So? The words are *put into the mouth of a heathen* (Cæcilius), but by a Christian, precisely as here they are put into the pen of an unbelieving Jew (Josephus), and with equal fitness. The cases are perfectly parallel: the term is not improbable, it is even probable in a forgery foisted on Josephus.

Harnack thinks the use of *hēdonē* "in the highest degree surprising." But we have just seen that there is nothing at all surprising in it, that not only is it guaranteed by II Clem. 15, but also by the New Testament *hedeōs* in the same connection, the two terms differing exactly as "with gladness" differs from "gladly."

6. Another phrase that surprises Harnack is that "many Jews and many too of the Hellenic race Jesus brought over to himself." He thinks every Christian sadly knew better, that Jesus worked only amid Jews, etc. Herewith Harnack assumes his own view of Christian origins as correct, whereas it is very remote from correctness. But in any case the spread of the Gospel to the Gentiles was much the most imposing part of Protochristianity, and nothing was more natural than to throw the fact back upon the (alleged) great founder. Why, is it not a commonplace of criticism that the life of the early Christian community is constantly reflected in the Gospel narratives about Jesus? What liberal critic calls this in question? Moreover it is stated distinctly in John xii. 20ff. that "some Hellenes of those that went up to worship at the feast" said, "We would see Jesus," and this is accepted by the Jesus as an occasion to declare that he would draw all men to himself (verse 32); and even in Mark i. 37 it is said to him that "all are seeking thee." Granted that all of this taken literally is unhistorical but designed only to set forth the universal conquest of the Jesus-cult, it is still a most ample foundation for the modest Christian statement in Josephus.

Such are the six pillars of Harnack's contention, and they all slip into ashes at the lightest breath of scrutiny. In his eagerness to defend "the historical Jesus," he has shut his eyes to the most obvious matters of fact.

Harnack next proceeds to analyze the Josephine passage and inquire of each clause, whether it *might* not have been written by the Jewish historian. Well, suppose they did *each* sustain the examination—would it indicate that Josephus had actually written it? By no means! It would merely show either that he had written it, or that the forger had been careful to say only what Josephus might have said. But which? There would be nothing[9] thus far to decide, other considerations must be adduced. Not that Harnack does succeed in showing the Jew might have written *each* (even this would not prove he might have written *all*), but merely that the argument is not advanced, even if he should prove it. Only one point need be noticed. The declaration "This was the Christ" Harnack takes to mean "this was the so-called Christ." Why then did not Josephus say so, since according to Harnack he actually does use these words in *Ant.* XX, 1, 9? The whole of Harnack's interpretation is thus exceeding strained and artificial, but it would take too long to treat it minutely, nor is it required. Only be it noted that Harnack admits that he is uncertain about the clause "for he appeared to them on the third day again alive, the divine prophets having spoken both these and myriad other wondrous things about him." Here, he confesses, is "a serious difficulty." He thinks we may get over the resurrection by remembering that "Josephus like nearly all his contemporaries was a believer in miracles" (*wundergläubig*), and that similar stories were told of Apollonius of Tyana,—a venerable slander. But "the prophets etc.," he thinks hard to understand, and perhaps, as Ranke thought, really an interpolation; nevertheless, he reluctantly undertakes a defense, by imagining that Josephus meant to say that all the prophecies were really fulfilled in Jesus, but that the hopes based thereon were only illusions, for the prophesied Messiah was crucified and that was the end of Jewish political aspirations. Such an attitude towards prophecy Harnack admits was "complicated," though he thinks it not impossible. As he lays little stress on this phantasm, it seems needless to discuss it.

But what of the other evidences of spuriousness hitherto held to be decisive, such as the obvious want of connection with the preceding and succeeding sections, and the ignorance of this passage displayed by Origen? Is it not surprising that Harnack has passed all these by in total silence? Even if we concede everything to his

[9] Except indeed the James-passage, the spuriousness of which does throw grave doubt on the kindred passage under consideration.

analysis and synthesis, why does he not mention the very proofs that are regarded as decisive? It is superfluous to repeat these proofs. They are set forth sufficiently in *Ecce Deus,* still more minutely by Norden. They are convincing to such as Linck and Corssen, Windisch, Zahn, McGiffert, Niese, not to speak of elder critics, who would have all possible interest in discrediting them, if by any means possible, nor even of the Benedictine editors of Origen, who seem to go out of their way to discredit the passage (I, 362, n.). Such are the proofs that Harnack does not even hint at. The reader has now before him the argumentation of the "well-trained dialectician" and may estimate its valor and its value.

But what has Burkitt to say? In a paper much shorter than Harnack's, published in the *Theol. Tijdschr.,* II, 1913, 135-144, as well as in his earlier work, *The Gospel History and its Transmission,* Burkitt not only supplies Harnack with his general construction of Josephus's frame of mind, but also feels the necessity of saying something about Origen. The Cambridge Professor thinks this Father's information about Josephus was inaccurate, that he had mixed up things in his commonplace book, that he did not think the passage "proved" anything, and especially that he was unfamiliar with the *Antiquities,* since elsewhere "his reference to the mention of John the Baptist is equally slipshod." [Not at all; Origen cites merely the relevant.] Burkitt thinks Origen may have sold his Josephus (along with other profane books), not for a mess of pottage but for an annuity. On the whole, the Syriac master's paper seems more valuable than Harnack's, less openly partizan and prejudiced. Conybeare, of course, has adopted, as a matter of fact, its modest conjecture about Origen's mixing up in his commonplace book the two notices in Josephus about Ananus's murder of James and his own murder, but in spite of this there may be some correctness in the conjecture. Something similar in the Benedictine note to *Or. c. Cels.,* I, 47: "So perhaps when Origen and others read in Josephus, *Ant.* XX, 8, that the death of James displeased many, afterwards by lapse of memory they thought Josephus had ascribed the overthrow of Jerusalem also to the murder of James."

However, the explanation of Origen's silence concerning the Christ-passage is very far from satisfying. Here there is occasion for historic imagination, such as Burkitt elsewhere displays. Origen was by odds the most learned Christian of his day, one of the most learned of any day. He was engaged in the closest grapple with the

pagan Celsus, no mean antagonist; he was certainly acquainted with Josephus, whom he cites repeatedly, even this very eighteenth book of the *Antiquities*. In his death-grip with Celsus he puts forth his utmost powers, he summons up all the resources of his knowledge and intelligence; he deploys many forces far less effective than the Josephine passage; indeed, there was no other passage anywhere that would have served his controversial purposes so admirably. If, then, he knew of the Josephine section, and regarded it as genuine, it seems incredible that he should have passed it over, that he should have urged so explicitly against Celsus the worthless contemporary testimony of Josephus to the Baptist, in Ch. 5, and have omitted this extremely valuable testimony to Jesus, in Ch. 3, *less than* 300 *lines before*. Even if he had not been familiar with the *Antiquities*, he would have hunted it up, for he must have known that it was the standard work on that period and people, and for him to neglect it would be like neglecting Thucydides in studying the Peloponnesian war. In spite then of Burkitt and his interested assertions, the silence of Origen must cast the darkest doubt on the Christ-passage.

How strong is prepossession and tendency even in Burkitt is vividly shown in the footnote, p. 120, where he would show that Josephus was just the man to believe that Jesus might "appear to his disciples alive": "Note that in *Antiq.*, XVII (end) Josephus tells how the dead Alexander appeared to Glaphyra who had been his wife." Is this quite fair to the reader? Turning to the passage in question we find these words: "And while cohabiting with Archelaus she beholds some such dream as this: She seemed, having beheld Alexander standing near, to rejoice and to enclasp him eagerly, but he made complaint of her and said: [follows his reproachful speech]. Having recounted this to her intimates among the women, after a few days she dies." The reader sees that it was only such a dream (*onar*) as every one has frequently, and in no measure justifies the language of Burkitt. Had the distinguished Syriacist glanced back a few lines, he would have found Origen (*C. Cels.*, I, 46) clearly distinguishing between *hypar* (waking vision) and *onar* (dream).

The Cambridge Professor commends Whiston's Dissertation I as "the best discussion of the problem I have ever read," and approves his idea, revived by Harnack, that Tacitus (*Annals*, XV, 44) derives from this Josephus passage—a notion thoroughly exploded by Norden and Corssen. Burkitt might well prefer Whis-

ton ("paradoxical to the verge of craziness, intolerant to the verge of bigotry") even to Harnack, who takes thence so much, but the Dissertation remains full of naïveté and flimsiness. One thing in it, however, is still valuable: Whiston distinctly and repeatedly admits that it is "almost incredible" that Josephus should have omitted all reference to an historic Jesus. This argument had great weight while the historicity was unquestioned; now, however, the inference is exactly inverted: from the silence of Josephus we deduce that it is "almost incredible" that Jesus was historic.

Neither Harnack, nor Burkitt will admit that his interest in the Christ-passage is due to the necessity of defending the historicity. Oh, no! God forbid! Harnack's sole interest is in the unlovely personality of the Jewish historian, and he assumes that "the historic Jesus" needs no defense! Burkitt ascribes the re-opening of the Josephus question to Berendt's discovery of a Slavonic version, which is everywhere justly recognized as a still more elaborate forgery and is scarcely mentioned in recent discussions! All this is very amusing.

Queer that Christendom should rally to such needless defense! One is reminded of Bret Harte's delicious couplet in "The Stage Driver's Story":

> "I have seen danger? Oh, no! not me, Sir, indeed, I assure you:
> 'Twas only the man with the dog that is sitting alone in yon wagon."

Prof. W. Emery Barnes, writing in the *Contemporary Review* of January, 1914, is not only more exact and critical but also more candid than some others; for he states that this "testimony is of no little value at the present day," since this early Jewish witness "supplies testimony of a unique kind to some of the outward facts of the beginning of Christianity, and he blocks the path of those who try to think that 'there is no historic reality' in the figure of Jesus the Christ." Whence it seems that no one really thinks it, though some "try to think it," the dishonest dreamers! What immense gains come to them that "try to think" so, and how noble and self-sacrificing are their opponents, the historicists, will appear distinctly from the following lines from the pen of one of the most illustrious biblicists of this or any other age (I translate from the German): "*Uebrigens* I consider any public discussion of the questions raised by you as not advisable. Moltke hinted once that every expert knew well that not the Crown Prince, afterward Emperor Frederick, but Chief of Staff Blumenthal had won the victories;

but that would not do for the masses; the idea of the victorious Crown Prince was worth a whole army. Therefore neither can you count on any public assent on the part of the representatives of New Testament exegesis in German universities; a German professor of theology who openly agreed with you would have to resign his position. Might he be ever so deeply imbued with the correctness of your views, he would never admit it."

Recent events in German clerical life illustrate the foregoing vividly; however, I can not quite concur. Motives not utterly noble need not be mercenary. Theologians and others feel that they have ancient and venerated interests to guard. If forced to capitulate, it must be on exceedingly honorable terms; the evacuation must look like a voluntary change of base, effected with all the pomp, pride and circumstance of glorious war. Never will they accept the "weird" notions of the "non-professional"—until they rediscover and redemonstrate the same or similar notions in their own minds by their own methods. Such is the decorum of criticism.—The story goes that an able and excellent pastor of a small flock in a country town received a "call up" to a large church in a great city. Naturally he hesitated. Such invitation to a wide and fertile field of usefulness could not be lightly regarded, yet it was hard to break the ties that bound him to his devoted fold. Some friends passing by his residence asked a little child playing on the lawn if the call would be accepted. "I don't know," was the reply; "it's mighty hard to tell. Papa is upstairs now, on his knees, wrestling with the Lord in prayer, and seeking light. But Mama is packing the trunks."

But in any case, when it is scoffingly asked by Harnack and the rest, "Have any of the specialists believed in him?" it is enough to point to the foregoing from a very chief of specialists.

On page 58 Barnes admits that "the doubts survived" the defense by Whiston in 1736; from which the reader would with difficulty deduce that for one hundred years before Burkitt the doubts, not of the genuineness, but of the spuriousness had been steadily disappearing, until "no reputable Protestant scholar continued to defend it" (Harnack), or that even the Benedictine editors of Origen surrendered the passage as spurious. ("The celebrated passage of Josephus concerning Jesus Christ was not extant in the age of Origen, or if extant it was considered spurious and supposititious," I, col. 1911). It takes time and space to note such details, but how else can you discover and uncover the method

and spirit of historicism? It is only since the "testimony" has recently appeared to be "of no little value," in fact indispensable "to block the path" of advancing knowledge, that so many have risen up to discover that it is certainly genuine and impossibly interpolation!

Barnes's arguments, presented clearly, forcibly, systematically, concern themselves mainly with points already sufficiently considered. He analyzes, like Harnack, and finds various things that "could not possibly come from a Christian pen. Could a Christian, who would naturally describe our Lord as the *Saviour of men who received him,* really chill himself down to write 'a teacher of men who receive true words with pleasure'"? Why not? Barnes seems to forget on every page that the Christian interpolator was not a fool, but was more or less clever, that he was impersonating Josephus, and that it was quite proper to appear somewhat reserved and chilly. Barnes appears to understand the feelings of all third century Christians perfectly, and he argues like a man who would defend A charged with forging C's signature by saying: "This can not be a forgery of A's, it must be C's own writing; for I know A's genuine signature and the signatures of all his family, and none of them resemble this present one in question." Such reasoning arouses wonder if not admiration. Barnes seems also to forget that this very "impossible" sentence has notable similars in the New Testament, as already exhibited.

Barnes rejects Whiston's translation, "the truth," as "peculiarly unfortunate" and substitutes "true words,"—still more unfortunately, for it leaves out the article; *t'alēthē* means not "true talk" but "the true," *das Wahre,* as Harnack correctly renders it. Similarly, in the clause "if indeed it be proper to call him a man," he renders *eige* by "since," thus: "Since it is befitting," which he would support by referring to "Liddell and Scott, s. v. *ei.*" Now in the sixth edition (1869) we do read that *eige* "often passes with the sense of *since,*" but not in the eighth, where it is only said "sometimes nearly = since." The passages cited by L. & S. (in the sixth) do not warrant the statement quoted, hence they are omitted from the eighth edition; nor are they now so rendered by the best translators. In all the cases the meaning is "if indeed," and so they put it. To be sure, where the indicative is used the conviction may be very strong, but this lies in the circumstances of the case, not in the form of speech. Professor Burkitt must revise his interested rendering.

He fails to catch any echo of the New Testament in the passage. It would signify nothing if there were none; the Christian interpolator might or might not echo the Christian scriptures. As a matter of fact, the assonances may be faint, but they are audible and in number. Thus *ginitai,* used of a person in the sense of "appears on the stage," "*tritt auf,*" is exceptional and suggests John i. 6 ("Came a man"), 2 Peter ii. 1 ("Came also false prophets"), I John ii. 18 ("Antichrists many have come"), also Mark i. 4 ("Came John the Baptist"). Barnes translates this *ginetai* correctly by "ariseth," Harnack most incorrectly by "lived" (*lebte,* instead of *tritt auf*). The usage smacks distinctly of the New Testament.—"That received the true with pleasure" has already been shown to be evangelic; "the chief men" is not unlike "the head men"[10] of the scripture. "They that loved him at first ceased not" goes well with "having loved his own that were in the world, he loved them unto the end"; the "myriad wonders" rhymes with "the world itself could not contain the books written" of "all that the Jesus did." Also the word uncanny, incredible,[11] used of the works done by Jesus is exactly that put into the mouth of the bewildered folk[12] (Luke v. 26). In the presence of these facts, is it possible to deny that this passage is colored Christian? What other paragraph is like-hued in Josephus? As to whether the general style is Josephine, that being so largely a matter of individual literary feeling, it is perhaps vain to argue. To me it seems not.

It has long appeared hard to believe that Josephus wrote, "This was the Christ," but now Barnes assures us it is certainly "not a Christian confession, the natural form of which would be 'This *is* the Christ.'" Since then it can neither be Christian nor non-Christian, what pray can it be? But why should not a Christian have written, "This was the Christ," a Christian personating Josephus and writing of the distant past? How ridiculous would "this *is* the Christ" have sounded! Barnes seems to forget that in Rev. i. 8 it is declared "I am the Alpha and the Omega, saith Lord the God, which is and which was and which is to come," also that "the Apostle" says "the rock was the Christ," which must mean "the Christ was (or constituted) the rock," and that "God was in the world reconciling etc."

Barnes's arguments about Origen have already been met in

[10] ἄρχοντες.
[11] παραδόξων.
[12] "We have seen παράδοξα to-day."

dealing with Burkitt. He seems to forget that this Father had no especial motive for quoting other things from Josephus, but that he had urgent motives for quoting this passage, for it bears strong testimony to the general historical situation, which Origen had been called on to champion against Celsus and which in that 47th chapter he did actually champion. When Barnes asks what motive a Christian could have in foisting off such a cool and non-committal testimony upon Josephus, the answer is obvious: It was the absence of historic witness to the biographical theory of the Gospel that galled the Christians seventeen and eighteen hundred years ago, *even as it galls them now.* They were intensely concerned to form a biographic substructure for their faith, just as Barnes and Harnack and historicists in general are now compassing sea and land to make one point in favor of their "historical Jesus." Their preferred method was to *make* the testimony they needed and to put it where it would do the most good. Hence the countless Christian interpolations and spurious writings that besprinkle the literature and becloud the history of those early centuries.[13] Does any one doubt this general statement? Let him hear the word of Corssen, who is happily untinctured with the anhistoric heresy: "But the disposition to fabricate documents by which the Christian faith was attested also in extra-Christian circles, shows itself very early. We need only think of the correspondence between Jesus and Abgar of Edessa (ca. 209), of the Acts of Pilate, to which Justin refers his readers for confirmation of the Christian tradition, or of the letter cited as early as Tertullian of the Emperor Marcus Aurelius concerning the rain-miracle that happened to the thundering legion" (p. 129).

It makes no difference at all whether the subtleties of Barnes be just or unjust; the question is not whether this "testimony" is valuable to the Christian in strict logic—why, admittedly the Christians made eager use of Old Testament "testimonies" that had no relevancy whatever—the question is this: Did this testimony *appear* to the Christian to be important? For if so, then he had a motive, logical or illogical, but in any case perfectly adequate, to account for his forging it, and perfectly sufficient to induce him to forgery. Did the Christians then think this "testimony" was to the point? Fortunately we can answer decisively, *they did.* For Eusebius

[13] For evidence to what extraordinary extent and infinite detail this ἐπανόρθωσις of documents was carried, see Norden's *Aghostos Theos,* pp. 122-124.

(A. D. 324-330) quotes the testimony in full, and each time *argues from* it vigorously (in *D. Ev.*, I, 11, and in *H. E.*, I, 11), saying in the latter case: "And since this writer, sprung from the Hebrews themselves, hath delivered in his work these things concerning John the Baptist and Our Saviour, what excuse is there left etc.?" Also Ambrose or Hegesippus (A. D. 360) writing on the destruction of Jerusalem (II, 12) describes Josephus wittily as "an author not to be despised when he writes against himself," quotes the famous passage with proper embellishment at the end, and then argues that the Jews must at least believe their own writers, that their historian's "testimony" is all the more weighty because unwilling and coming from a hard-headed unbeliever. The like may be said of Jerome (A. D. 400), of Chrysostom's pupil Isodorus (A. D. 410), of Sozomon (A. D. 440), of Cassiodorus (ca. 510), and so on down through the long list of writers that have occasion to use this witness. All of these Christians are delighted beyond measure with this "testimony," it seems to suit their purposes precisely. The very clause that Barnes thinks "no Christian could have written," Isodorus cites with especial wonder and admiration. Plainly then Professor Barnes has entirely misunderstood the early Christian consciousness, and all his hair-splitting has been love's labor lost.

Lastly, all of these new defenders miss the main point of the matter, when they pass by in silence the want of connection between the passage and its context, the disjunction both before and after. This has been specially stressed in *Ecce Deus* (pp. 232, 341), later also by Norden, who cites Faber as having also mentioned it. I am pleased to note (to-day for the first time) that McGiffert also years ago rightly judged this consideration to be decisive.

Well, then, the recent knight-errantry fails at *every point* in its championship of the Josephine witness; all its "spearshafts are shivered in the first onset." The passage stands out much more clearly than ever before as interpolation. If so, then is the minor passage about "the so-called Christ" also interpolated; then Josephus has never mentioned "the historical Christ," and this says Whiston, and as so many others like Steck have felt, is "almost incredible" if the Jesus was an historic man; hence again, by the admission of historicists themselves, it is "almost incredible" that Jesus was historical.

Hence the exceeding importance of the whole discussion, hence the desperation with which the "testimony" is defended. Still more,

however, the reader can not have failed to note how frequently faulty are the historicists both in fact and in logic. Why is this? Why do these consummate scholars mar their pages with blunders that even a schoolboy, nay, even an amateur may detect and expose? There is but one answer. They are blinded by passion. Their gift of tongues, their mastery of history, their subtlest analysis, their most ingenious synthesis—all are of no avail; they can not reason aright, they can not translate faithfully, they can not state facts correctly. They are set for the defense of foregone conclusions—as Harnack naively writes: "nevertheless the defense of the sentence seems to me to be not altogether hopeless, and I will try to conduct it"—and their presentations are as scientific and as free from bias as a Catholic priest's who seeks *imprimatur* from His Holiness.

TULANE UNIVERSITY. WILLIAM BENJAMIN SMITH.

THE SIGNIFICANCE OF THE HISTORICITY PROBLEM.

To the Editor of The Monist:

I wish to express my sincere thanks to you in the first place for opening your pages to a discussion of the Jesus-question,[1] and especially for allowing such a full expression to views in which you yourself do not share. There is a growing interest in this subject in Great Britain, and I hope and believe that there will be an increased demand for publications that discuss it.

I wish also to thank you for your contribution to the discussion. I find myself in full agreement with the conclusion to which you come; and not only so, but it is the conclusion in which I am specially interested. As to the question whether there ever was an individual, personal Jesus, I cannot say that I have much interest, nor do I believe that it is a question that can ever be decided. I thought I had stated this point with sufficient clearness in the course of my paper, but it seems that I did not. I did not mean to make the impression that the all important point to me is the question of the historicity of Jesus, as though I wished above all things to prove that no Jesus ever existed! It is so difficult to make one's meaning clear, and unfortunately we must use words

[1] This letter has reference to the preceding number of *The Monist* (July 1914) which was devoted to the subjects of tragedy and the historicity of Jesus. Among other contributions the number contained one by the author of this letter on "The Person of Jesus Christ in the Christian Faith," and another by the editor entitled "The Religion of Tragedy and the Christ-Ideal."

which do not convey our meaning, do what we will. For this reason the very word "historicity" is misleading. I would admit at once that there may have been a historical Jesus; but the point is that he was *not* the significant thing for Christianity, and this, as I understand it, is your conclusion.

The significant thing for Christianity is the timeless, super-historical truth of "dying to live," of vicariousness, which you rightly show is inwrought into the constitution of human life and society. I think that it might be carried a little farther, viz., that it is inwrought into the structure of the universe itself. The cosmos may be the result of sacrifice. A thing is a thing by virtue of its limitation, and limitation is sacrifice. Manifestation is limitation or sacrifice. The old mystics used to say that "creation is the Calvary of Deity," that there could not be a creation without a cross. What, therefore, you show to be true of human life and the destiny of mankind may be true also of the Supreme Spirit itself, or of the Source from which all things come, whatever name we may give to it. This is the essential thing in Christianity, and I think it is supremely important that this should be seen. It is this that gives the Jesus question any interest to me; the mere question apart from this, whether there ever was a man Jesus is to me one of absolute indifference.

Nevertheless I find it exceedingly difficult to make people understand my position, and in spite of all I can do they attribute to me the "spirit of denial." Perhaps I may be allowed to mention an illustration of the difficulty of making my meaning plain: Some years ago I wrote an article for the *Hibbert Journal* under the unfortunate title of "The Collapse of Liberal Christianity." I thought I made sufficiently clear the sense in which I used the word "collapse," namely the failure to find a Jesus who would do for the founder of Christianity. I pointed out that the liberal critics cannot find the Jesus they put at the foundation of their conception of Christiantiy. The Jesus others profess to find they would not thank them for. He would not serve their purpose. The Jesus they posit is a creation of their own imagination.—Well! Prof. Benjamin W. Bacon answered my article, taking me to mean that modern thought has suddenly come to a standstill, and giving reasons why I was mistaken in so thinking!

I now write an article on "The Person of Jesus Christ in the Christian Faith," and you think that I wish to prove that there never was a Jesus! Again I have failed to make my meaning clear.

Behind all the symbolism of the New Testament there may have been a Jesus; I am inclined to think there must have been. But all the same that is not the important thing in the New Testament, nor the most important thing in Christianity, and this is the whole point of your contribution to the subject. I can understand why the Unitarian clings to his "man Jesus." Orthodoxy, or the soul of orthodoxy, may survive after this discussion, but the Unitarianism which is founded on the "man Jesus" is doomed.

K. C. ANDERSON.

DUNDEE, SCOTLAND.

FIFTH INTERNATIONAL CONGRESS OF PHILOSOPHY, LONDON, 1915.

By a resolution passed at the Fourth International Congress of Philosophy, held at Bologna in 1911, an invitation from the University of London to hold the Fifth Congress in London in 1915, with the offer to provide accommodation for the meetings in the buildings of the university was accepted, and the president and the secretary of the congress were appointed.

A general committee of organization has been formed, consisting of the leading students and teachers of philosophy in Great Britain and Ireland, and of representatives appointed by the British Academy. This committee has appointed an executive committee.

The King of England has consented to be patron of the congress, and Lord Haldane, the Lord High Chancellor, has accepted the position of honorary president. The president is Dr. Bernard Bosanquet, the honorary secretary is Dr. Wildon Carr, and the honorary treasurer is Dr. F. C. S. Schiller.

The congress will open on Tuesday evening, August 31, 1915, at the University of London, at eight o'clock, with the presidential address, after which there will be a general reception. The work of the congress will begin on the following morning, and will be continued from day to day until Tuesday, September 7, when the congress will close. The meetings will be held at University College, the general sessions in the mornings from ten till one o'clock and the sectional meetings in the afternoons from two-thirty to five. There will be no meetings on Saturday afternoon or on Sunday, but excursions will be arranged, and will include visits of members to Oxford and Cambridge. Luncheon and tea will be

obtainable during the congress week in the refectory of the college. The whole congress will be accommodated within one building so that the inconvenience due to separation of sections in different institutions will be avoided. It will also be possible, if desired, to hold other meetings during the congress besides those in the regular program. A public dinner, open to all members of the congress and to a limited number of guests, will be arranged to take place on Monday evening. A list of hotels and boarding houses conveniently situated in the neighborhood of University College will be sent to all who have given notice of their intention to attend the congress, and if possible, arrangements will be made for special reductions on railway tickets.

The general sessions will be devoted to the discussion of subjects of wide philosophical interest. The subjects proposed are:

1. "The Nature of Mathematical Truth." In this Professors Royce, Brunnschvieg, Mr. Bertrand Russell and others will take part.

2. "Life and Matter." Professors Bergson, Driesch, Verworn, and Lord Haldane will take part.

3. "Realism." This is the only symposium which is as yet fully arranged. Professors Perry, Meinong, Husserl, Strong, and Alexander will take part in it.

4. "The Philosophy of the Unconscious." Dr. Jung and Dr. Morton Prince will take part in this, and others have been invited.

5. "Pragmatism." Professors Boutroux, Dewey, Enriquez, and others have been invited.

Each subject will be introduced by a symposium of four or five short papers representing different points of view. It is hoped to have all these arrangements for symposia complete in a few weeks, and the introductory papers will be published in a volume to be presented to each member of the congress at the opening. The papers will not be read out at the meetings, but the writers will open the discussion by short speeches giving an analysis of their papers, or developing the point of view put forward, or criticizing the other papers, after which the discussion will be general.

The sections are being organized, each under a president, as follows:

I. General Metaphysic. President, Prof. G. Dawes Hicks.
II. Theory of Knowledge. President, Prof. S. Alexander.
III. Logic. President, Dr. A. Wolf.
IV. History of Philosophy. President, Prof. W. R. Sorley.

V. Psychology. President, Dr. C. S. Myers.
VI. Esthetics. President, Prof. J. S. Mackenzie.
VII. Moral Philosophy. President, Prof. J. H. Muirhead.
VIII. Social Philosophy and Philosophy of Law. President, Prof. L. T. Hobhouse.
IX. Philosophy of Religion. President, Prof. A. Caldecott.

An oriental section or sub-section is under consideration. Proposals to read papers cannot be considered later than April, 1915, but to avoid disappointment early notice is requested, as it is only possible to accept a limited number of papers.

Every member of the congress, whether attending as a delegate or by invitation, or in a personal capacity, subscribes the sum of £1. Ladies are admitted as full members; or, if accompanying members of the congress, they can become associate members on payment of half the subscription. Associate members will be entitled to attend general and sectional meetings, but not to read papers nor to receive a copy of the *Proceedings*.

All communications should be directed to the Secretary of the Congress, Dr. H. Wildon Carr, More's Garden, Chelsea, London, S. W. In particular, it is especially important to inform the committee, as soon as possible, if an intending member proposes to contribute a paper, and to indicate on what subject and for what section it is destined.

Checks and postal orders should be made payable to Dr. F. C. S. Schiller and sent to him at Corpus Christi College, Oxford.

In what precedes, some particulars other than are contained in the printed announcements have been kindly furnished by Dr. Wildon Carr.

In conclusion it may be mentioned that the American members of the Permanent International Committee of the congress are: Prof. J. M. Baldwin (New York), Dr. Paul Carus (Chicago), Prof. G. Creighton (New York), Prof. G. S. Fullerton (New York), Prof. G. T. Ladd (New Haven), Profs. H. Muensterberg and J. Royce (Boston), Prof. J. G. Schurman (Ithaca), and Prof. C. A. Strong (New York). Φ

CURRENT PERIODICALS.

The number of *Scientia* (*Rivista di Scienza*) for July, 1914, begins with an article by Bertrand Russell on "The Relation of

Sense-Data to Physics," which is of special interest in connection with the author's "Lowell Lectures" of 1914 on *Our Knowledge of the External World as a Field for Scientific Method in Philosophy*, which have just been published by the Open Court Publishing Company. Physics, says the author, exhibits sense-data as functions of physical objects, but verification is only possible if physical objects can be exhibited as functions of sense-data. Further, in so far as physics leads to expectations, this verification *must* be possible, since we can only expect what can be experienced. We have therefore to solve the equations giving sense-data in terms of physical objects, so as to make them instead give physical objects in terms of sense-data. This problem leads to much interesting logico-mathematical work. In this paper, a rough preliminary sketch is given. H. A. Lorentz examines those theories of gravitation whose aim, as distinguished from mechanical theories of gravitation, is to bring gravitation into connection with other phenomena and to imagine the nature of the bonds which unite it to these phenomena. Three such theories are here spoken of: (1) Lorentz's own (1900) "electromagnetic theory of gravitation"; (2) The relativist theory of Poincaré and Minkowski (1906 and 1908); (3) The theory of Einstein (1907, 1911, 1914). L. Cuénot maintains that, considering the insufficiency of the classical explanations of adaptation by selection, it is natural to think of adaptation before entry into the environment; and the notion of pre-adaptation falls into line with that of mutations and with the Mendelian theory. Alfred Adler writes on the psychology of the individual, its presuppositions and its results. Raffaele Pettazoni shows that, from its beginnings to its full development, the history of Christianity is closely connected with the universal religious history of humanity. Max Abraham has a critical note on Einstein's article in the May number of *Scientia*. Franco Savorgnan has a general review on social antagonisms. Besides this, there are reviews of books and periodicals, a chronicle of future plans in the scientific world, and French translations of the German, Italian, and English articles.

The *Revue de Métaphysique et de Morale* for July, 1914, contains an address by Gustave Belot, delivered at the School of Advanced Social Studies, on the moral value of science. Etienne Gilson writes on the Cartesian theory of innate ideas and theology. Georges Dwelshauvers writes on religious feeling and its relations with art. G. Aillet has a critical study on Maxime Leroy's book *La Coutume ouvrière*. The "practical question" discussed in this

number is patriotism, and is by G. Siméon. There is the usual supplement containing reviews of books and periodicals, and accounts of recent theses for the doctor's degree at the University of Paris.

The number of *Isis* for June, 1914, is a bulky and interesting one. Yoshio Mikami writes "On the Japanese Theory of Determinants." It will be remembered, from David Eugene Smith and Mikami's *History of Japanese Mathematics,* published not long ago by the Open Court Publishing Company, that Seki Kôwa actually antidated Leibniz in the discovery of determinants. This article gives fuller details. August Georges-Berthier has a long and wonderfully thorough article on the Cartesian mechanical theory of the universe and physiology in the seventeenth century. Aldo Mieli writes on Vannoccio Biringuccio and the experimental method shown in his posthumous *De la Pirotechnia* of 1540. Waldemar Deonna gives some reflections on the theory of the origins of art in its relations with present tendencies. Emile Turrière describes the notion of geometrical transcendence with Descartes and with Leibniz. The indefatigable and self-sacrificing editor, George Sarton, gives (1) a general introduction to a synthetic bibliography of reviews and collections of books, (2) a list of 62 reviews and collections devoted to the history of the sciences. There are also a chronicle of scientific events, analyses of new books and articles, and a continuation of the important critical bibliography.

This journal is published in Belgium and is international. At the present moment, the understanding between nations that is so helped by science is broken by a brutal war whose storm-center is near where *Isis* comes from. Φ